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KING'S COLLEGE HOSPITAL.

CASES IN WHICH AMPUTATION WAS PERFORMED;

WITH

CLINICAL REMARKS.

BY

MR. FERGUSSON.

In a clinical lecture, delivered on the 16th of February, 1843, Mr. Fergusson stated that he would take the opportunity which then presented of making some observations on amputation and the mode of dressing stumps, as some of the cases which had been lately under treatment enabled him to do so in a much more satisfactory and useful manner than during the systematic lectures on such subjects.

Case 1.—David Owen, ætat. 7, of a strumous habit, admitted 10th of December, 1842; had suffered from disease of the knee since he was eighteen months old. From time to time the affection (gelatinous alteration of the synovial capsule, abscesses, chronic inflammation of bones, &c.) had seemingly yielded to treatment, and about twelve months ago the hamstrings were divided to facilitate the extension of the limb, which had become much bent at the knee during the progress of the case. In the course of several months the limb was made almost straight, but acquired no additional strength, and the apparatus which had been used for extension was still kept on to aid the patient in his attempts to support the body on this limb. About six months since inflammation again attacked the joint, and ere long an abscess burst on the outside of the knee. His health now suffered considerably, and the discharge was profuse. A probe passed into the opening came in contact with bare bone.

22. To-day amputation was performed in the lower third of the thigh, with an anterior and posterior flap. Four arteries were tied, and all bleeding having apparently ceased, the stump was stitched and strapped. About three hours after some blood was observed to trickle from the wound, and a clot protruded between the stitches. These were cut, and a large mass of blood having been removed, a small artery, which bled freely, was secured, when cold cloths were applied, to check the oozing from other vessels which did not seem to require ligatures. In the evening, the hæmorrhage having ceased, the cut surfaces, being covered with lymph, were brought again into apposition, and retained by stitches, straps, and a bandage.

23. Has passed a good night.

24. Child doing well; stump not disturbed.

26. Dressings of stump removed; looking well; slight swelling; scarcely any discharge; edges united. New straps applied. A strip of lint, covered with spermaceti ointment, laid along the course of the wound, and a bandage put on.

27. Stitches removed to-day. Straps, lint, and bandage applied as before. At each dressing the bandage is carried from the pelvis to the end of the stump, great care being taken to bring the soft parts well over the cut surface of the femur.

Jan. 16, 1843. A similar mode of treatment as that above described has been pursued. The stump is now almost healed, and the patient is about to quit the house, having rapidly improved in appearance and health.

Immediately after the operation Mr. Fergusson stated that this case had been under his notice for more than twelve months, and from what had occurred within the last six, as also from the disease having been present so long, he had resolved to recommend amputation. He believed that had he allowed the limb to remain there was great risk of the patient sinking under the constant irritation; and as by the process of extension the head of the tibia had been in a manner dislocated behind the condyles of the femur, it was very doubtful if the limb would ever have been of service; even in the event of all disease ultimately ceasing, he had no doubt that the course which he had pursued would be most to the advantage of the patient. This was the second instance in which he had amputated in the thigh some time after attempts had been made to extend the lim

by division of the hamstrings and the application of apparatus. One of these cases had been treated by himself, and in the case above related he had sanctioned the mode of practice. In both cases he had imagined that the original disease had ceased, and although he could not admit that in either of them the recurrence had been induced by the treatment, yet they bore out the general view that he had on sucha question, viz., that the practitioner should be very cautious in adopting anything like force to effect the extension, for whether the reappearance of inflammation was attributable to the surgeon's interference, the patient or his friends would be too apt to think so, and thus both the surgeon and his profession might suffer in character. The mode of amputation with a short anterior flap and a long posterior one, he had frequently alluded to on other occasions; and he would now request the pupils to watch the future progress of the case and the mode of treatment,—subjects on which he would dwell at greater length on another occasion.

Case 2.—Thomas Kallord, ætat. 10, admitted 26th January, 1843. Has had disease in the right tarsus since he was three years old. At present the part is more than double the natural size, and there are three orifices on the surface from which there is a constant discharge of matter, and through which a probe can be thrust upon bare portions of the tarsal bones. The boy is small and weakly, but at present is in as good health as he ever enjoys at any time. He has entered the hospital for the purpose of having the foot removed.

28. Amputation was performed to-day in the middle of the leg. A small anterior flap was formed by sweeping the knife in a semilunar direction from one side to the other, and then a large posterior one was made by transfixing in the usual manner. Three arteries were secured; the surfaces were held together by stitches; lint, moistened with cold water, was laid over the wound, and straps were ordered to be applied in a few hours, after all bleeding had ceased. In the evening the edges between the stitches were carefully approximated with straps, and a roller was then carried round the stump.

The subsequent dressings were similar to those used in the preceding case. On the fifth day after the operation, at the period of dressing, there was an escape of arterial blood, to the extent of an ounce or somewhat more, when the wound was partly opened by the finger, for the purpose of exposing the bleeding vessel. There was no occasion, however, for either pressure or ligature, as the blood immediately ceased to flow. Cold cloths were applied, and in the evening, as there had been no recurrence of the hæmorrhage, the surfaces were again approximated by straps. A constant watch was kept on the stump for several days and nights thereafter, but there was no further cause of alarm; the wound healed rapidly, and on the 25th of February was entirely cicatrised.

With reference to this case it was stated by Mr. Fergusson that when the amputation was performed there was no greater urgency in the condition of the foot than there had been for years before. He had himself seen the boy two years previously, and had then recommended the operation, for it appeared to him that there was no probability of the disease getting well, and that its continued presence would in many respects be prejudicial to the patient. The parents, however, would not listen to the proposal, and he had not seen the case during the interval. A few days before the boy came into the house he had been brought to him, with a request that he would now perform the operation, as both the parents and patient himself now despaired of a cure. Considering that as the disease had been present for seven years; that it was in no respect better than it had been two years before; that this constant source of irritation was checking the boy's growth, retarding his education, and the cause of much trouble and anxiety to his parents, he had not hesitated about the step which he had taken, more especially as he could perceive no means of treatment likely to save the foot. Amputation had been performed in the middle of the leg with the view that an artificial support should be afterwards used, which should permit the free use of the knee-joint.

Case 3.—Alfred Allen, ætat. 7, admitted December 16, 1842. Was always a healthy boy, though thin and weak-looking, until four months ago, when he was observed to limp in his walk, and complained of pain in the left knee, which gradually became much swollen. An abscess at last pointed in front of the head of the tibia, which was opened about two months ago, and the aperture through which bare bone can be felt, has never closed since. The swelling diminished for a time after the evacuation of the matter, but has again become very conspicuous, more especially on the upper and outer side of the knee, where fluctuation is very distinct. He has now constant pain in the joint, and is much disturbed in his sleep. The cartilages do not seem very tender when they are pressed, but there is so much inflammation around the joint that the parts can scarcely be touched without causing additional distress.

22. Abscess has been opened and a large quantity of pus evacuated. It does not seem that the matter has communicated with the joint; the cavity of the abscess, however, extends above the patella, from one side of the knee to the other. At a consultation to-day it has been decided not to amputate at present, as hopes are entertained that the joint is not seriously affected.

Feb. 4. Since last report the symptoms have become more severe, and, as the patient's health is giving way, it has been determined to remove the affected part. This has accordingly been done at the hour of visit to-day, by a method similar to that pursued in Owen's case. Eight arteries required to be secured; four interrupted sutures were used; lint and cold water were applied for four hours, and then straps and bandages were used, as in the preceding cases.

7. Stump dressed to-day; looks well, and apparently has healed by the first intention.

11. All the ligatures have come away to-day, and the stump is nearly healed. The boy's general health improving rapidly.

17. Stump quite healed, and the patient is about to leave the hospital.

With reference to this case it was observed that so long as hopes were entertained that the joint was not irrecoverably effected the amputation was delayed, but at last it became so apparent that the patient's health ould not long bear up against such irriation: hat further delay would have been culpable.

In speaking of both of these instances of amputation in the thigh, Mr. Fergusson took particular notice of the shape of the flaps, the posterior being purposely made considerably longer than that in front. From whichever side the flaps are taken, and whether the operation be by flaps or circular incisions, the posterior or lower side of the stump invariably, in the course of time, retracts much more than the anterior or upper portion, and ultimately the cicatrix is so drawn upwards and backwards as to give the stump a most unseemly appearance,—having the bone projecting in front, with a scanty covering of soft textures. If, however, care be taken to leave the textures on the back of the thigh longer than those in front, the retraction only brings the cicatrix to about the centre of the stump, and then the bone seems properly protected on all sides, and the part has that full and rounded appearance which is so character'stic of a well-formed stump.

In all these examples great care had been taken to secure every vessel which seemed unlikely to close spontaneously. It would be remarked, however, that bleeding had occurred afterwards in two of them. In the first (Owen, amputation in the thigh) it had taken place within a few hours after the operation, and this was a period when it was extremely apt to come on, however mindful the surgeon might be whilst the patient was on the operating table. The shock of the operation, exposure of the patient's body, and of the cut surfaces to the cold air, all tended to lessen the heart's action; but in the course of a short time after the person is put to bed, has probably had some cordials administered, and got warm under the bedclothes, the circulation becomes excited, and then one or more arteries which had been overlooked throw out their blood, and necessitate the further interference of the surgeon. Such had been the case here, and so often did it happen that in the estimation of some it formed a good reason for not dressing the stump until all chance of bleeding at this period had ceased. In the other instance of hæmorrhage (Kallord's case) the occurrence had come on at a more unusual period. Secondary hæmorrhage seldom took place until the ninth or twelfth day, but here it had appeared on the fifth. The mode in which it ceased was somewhat singular. It happened, fortunately, at the time when he (Mr. F.) was dressing the wound, and for an instant it seemed so profuse that he had applied pressure with the fingers upon the popliteal artery, which was kept up until he had gone the round of the ward, under the hope that by thus checking the current for a time it might cease to flow from the open vessel when the fingers were removed. This, however, was not the case, for it again flowed freely, and it then became a question what course should be followed. Considering that the union of the wound was still so recent, he had resolved, on the spur of the moment, to open it up again, with the intention of exposing the open vessel and applying a ligature; but the latter was not found necessary, for the bleeding ceased as soon as the surfaces were separated. This was different from his anticipation, as the current seemed at first too impetuous to be arrested by the mere exposure to the air; he was, therefore, inclined to suppose that the explanation, under the circumstances, was this,—that during the dressing some adhesions had been broken, and thus a vessel sufficient to yield a tolerable stream of blood had been partially laid open, the remaining adhesions preventing its retraction, and that when these had been broken up by the finger the vessel had retracted, and the hæmorrhage had then ceased in the usual manner. With such a warning he had placed a constant watch on the patient for the next two days, as the bleeding might have recurred during the night, and the consequences might have proved fatal.

In all these cases he had been careful at each operation to secure by ligature every artery that seemed unlikely to cease bleeding. Eight had required to be tied in Allen's case, while three appeared sufficient in Owen's, although another was afterwards found necessary. Both of these thighs were nearly of a size, yet eight ligatures were required in one and only four in another! This, however, was easily accounted for, as in Allen's case there was much greater swelling, more active disease, more inflammation, than in Owen's, and it was always found in such cases that the smaller arteries of the thigh yielded more blood than in instances where amputation was required for

accidents, or for diseases where the knee was not much enlarged.

It was his general practice not only to secure all vessels likely to bleed for any length of time, but also to apply all the usual dressings which he was in the habit of using on these occasions whilst the patient was on the table. On the latter point there might occasionally be reason for certain modifications, for instances would occur from time to time where the surgeon might apprehend further bleeding, or where, from the continued oozing from small vessels, it might be deemed most advisable to expose the surfaces for a time to the air; but in by far the greater number of instances he was still of the opinion that he had always entertained, that the best plan was to dress the stump at the period of the operation.

With reference to the means of keeping the surfaces in apposition, he gave the preference to stitches, straps, and bandages, as the pupils had seen them used on these occasions; the stitches were introduced in such numbers as he thought necessary—in one case four or five sufficing, whilst in another several more might seem requisite; these he allowed to remain on the stump for eight and forty hours at least, and where they appeared to cause little irritation he preferred retaining them for one or two days longer. The common adhesive strap he applied between the stitches, so as to cause the most accurate apposition; along the line of the wound he generally put a piece of lint, covered with simple dressing, and over all put a common roller. Sometimes, instead of the two latter, he applied lint, with cold water, especially where he was afraid of oozing of blood, or a high degree of inflammation, but generally the ointment and bandage were to be preferred.

The second dressing was usually on the second day from the operation, but if there was little discharge and no apparent excitement in the stump he often delayed this step till the third, or, occasionally, even the fourth. On such occasions, after removing the bandage and strips of lint, he generally cut two or three of the stitches, and perhaps took off several of the straps, especially if the latter were soiled and stretched, and then applied new straps, clean lint, and a fresh roller. In four and twenty hours similar steps were repeated, and then generally all the stitches and original straps were taken away. From first to last the dressings were as simple as they could possibly be. The straps and roller were of great service in giving support and form to the stump, whose ultimate condition depended greatly on the skill with which they were used; and the simple ointment was solely intended to facilitate the removal of the portion of lint with which the wound was covered. At first, both straps and bandage were applied loosely, but in the course of ten or fifteen days, *i. e.*, after the separation of the ligatures, they were so adjusted as to give both firm support and form to the part. In the thigh, unless the surgeon attended to the mode in which a stump was dressed, the part might assume a most discreditable appearance, however well it may have been made at first. Usually, in such cases, it was most easy for the patient to keep the stump bent upwards to the pelvis, and therefore it was generally propped up with cushions in this attitude; it was a dangerous one, however, if not properly attended to, as the weight of the soft parts, besides the natural tendency to contraction, caused them to descend towards the pelvis, and then the end of the bone might appear but scantily covered, or might project—even in instances where the flaps had originally appeared of most abundant length. In these cases, then, he carefully inculcated the propriety of holding the soft parts well up towards the end of the stump, before applying the straps and bandage, and also the advantage of beginning the application of the latter close to the pelvis, and bringing it by successive turns up to the end of the stump. After the lapse of three weeks there was little use for straps, but the bandage should not be given over for many weeks more—it was still useful in giving form to the stump, and was of much greater service in these cases than many seemed to imagine. Towards the final closing of the wound it might occasionally be deemed advisable to use an astringent lotion or stimulating ointment. Such practice had been pursued in some of these cases where the wounds had assumed the appearance of simple sores, but no particular comments on these points seemed necessary on the present occasion.

DUMOULIN, a celebrated French practitioner of former times, never lost sight of the fact that the payment of the fee is an essential part of the visit to a patient. If asked, "Shall you come again, doctor?" he would reply, "Yes, if you pay me." "Must I pay you now?" "Yes, if you wish me to come again!" Having, with a brother-physician been called to attend a nobleman who was dangerously ill, both doctors arrived at the house one day when the patient was *in articulo mortis*. The servants in the hall were full of invectives, and audibly threatened the disciples of Æsculapius with summary punishment in case their master should die. This catastrophe having taken place, Dumoulin's colleague, terrified at the impending penalties, anxiously inquired of him by what door they should get out. "By the door they pay at," said Dumoulin, and followed by the other, he boldly traversed the hall, and went to demand his fee.

ON THE

PHYSIOLOGY AND PATHOLOGY

OF

THE URINE.

By George E. Day, M.A., L.M., Cantab., Physician to Finsbury Dispensary, &c.*

The *quantity* of urine secreted in twenty-four hours varies considerably in different individuals, and even in the same person at different periods. Generally speaking, more urine is passed in winter than in summer, and in cold than in warm climates; this circumstance, of course, arises from the principle of antagonism which exists between the renal and the cutaneous functions. The standard quantity of urine voided in twenty-four hours has been estimated by Boissier at 24 ounces; by Hartmann at 28; by Prout at 32 (30 in summer and 40 in winter); by Lehmann at about 33; by Robinson (whose experiments upon himself were very carefully continued for a year), and by Christison at 35; by Gorter at 36; by Keil at 38; by Rye at 39; by Bostock at 40; by Becquerel at about 42; by Sanctorius at 44; by Stark at 46; by Dalton at 48.5; by Haller at 49; and by Lining at from 56 to 59 ounces. The latest observations upon this point are those recorded by Vogel in "Wagner's Physiology."

The mean quantity passed by a man during a tardy convalescence from a serious attack of disease, as deduced from careful measurement during a period of 189 days (from the middle of November to the middle of May), was very nearly 26 ounces. The largest quantity discharged in any day was in November, when 55.5 ounces (more than double the amount of the mean) were passed. The smallest quantity discharged in any day was in April, when 14.5 ounces (little more than half the mean) were voided.

In a state of perfect health, with ordinary exercise and diet, we may safely estimate the average quantity of urine at from 32 to 42 ounces. Becquerel places the limits between which, in a state of health, the quantity should range at 28 and 47 ounces. This is, however, a somewhat hasty statement, and the results of Vogel are sufficient to show its inaccuracy.

The *temperature* of the urine in adults is usually about 94°. It is stated, however, to vary from 75° to 112°, or even 120°. The extremes, according to Rayer, occur in the cold stage of intermittent fever and in scarlatina; according to L'Heretier, in peritonitis and variola.

The temperature of the urine of adults is usually a few degrees higher than that of children or aged persons. This observation was first made by Magendie, and has since been confirmed by the experiments of Chevalier and others. The same observation holds good with respect to the specific gravity.

The *specific gravity* of the urine is subject to as great deviations as the quantity, and the average standard has in consequence been very differently estimated. D'Arcet states that the specific gravity of healthy urine may vary from 1001 to 1060. Chossat observed it as low as 1001.6 at a temperature of 16° C. (or 61° F. nearly) after coming out of a cold bath, and as high as 1038.

The limits assigned by Cruickshank were 1005 and 1035, which exactly coincide with those subsequently given by Gregory from 363 observations.

The mean specific gravity deduced from these experiments of Gregory was 1022.6. I do not recollect that it is stated at what temperature these observations were made; we may, therefore, conclude that the recorded densities are those taken immediately after the emission of the urine. The average of upwards of 400 observations made by myself upon adults (principally males) is 1023 at the period of emission, or 1027 at 50°. The density is usually the highest about four or five hours after dinner. Upon instituting a series of experiments upon the diurnal variation, which were continued for one month, I found the mean density to be 1021; at two, p.m., 1022; and at eleven, p.m. (about four hours after dinner), as high as 1028. In this series of observations the urine was allowed to cool to 50° before the density was taken; and in all other cases referred to in these papers a similar course has been pursued. We not unfrequently find it stated that the urine of females is of a lower density than that of males. My own observations lead me to believe this statement to be, upon the whole, correct: Becquerel's observations, however, upon a limited number of cases, give an opposite result.

Having observed the density, the next point is to ascertain whether the urine is acid, alkaline, or neutral. In ordinary cases a couple of slips of test-paper will furnish us with all the required information. There are, however, frequently cases in which it is of importance to estimate the quantity of *of free acid* in a given quantity of urine. The method which I usually adopt in such cases is to take equal quantities of urine and pure water, to add to each an equal quantity of tincture of litmus, and then to observe the quantity of acetic acid (of known strength), which must be added "guttatim" to the watery mixture until a tint of equal redness is observed in both glasses.

The urine is a secretion of an extremely compound nature, and even at the present day chemists are by no means agreed with regard to certain elements, whether normally

* These papers will contain the substance of an essay to which the Harveian medal for 1842 was awarded.

they are constantly present, or whether they are merely due to adventitious causes.

In one of the best and most recent works upon animal chemistry, namely, that of Simon,* the constituents of the urine are arranged in the following order:—

1. Water.
2. Urea
3. Uric acid.
4. Extractive matter.†
5. Mucus.
6. Hœmaphœin (the brown colouring matter of the urine).
7. Uroerythrin (the red colouring matter of the urine).
8. Carbonic acid.
9. Lactic acid.
10. Hydrochloric acid.
11. Sulphuric acid.
12. Phosphoric acid.
13. Silicic acid.
14. Hydrofluoric acid.
15. Soda.
16. Potash.
17. Ammonia.
18. Lime.
19. Magnesia.
20. Oxide of iron.

As many of these substances may be conveniently arranged in groups for consideration, we shall adopt the following arrangement in treating of the various constituents of the urine:—

1. Water.
2. Urea.
3. Uric acid and the urates.
4. The inorganic acids, with their salts.
5. Extractive matter, lactic acid, and certain lactates, mucus, colouring matter, &c. &c.

Water.—The only method of ascertaining the proportion of water in a given quantity of urine is by direct evaporation. If the specific gravity of the residue after evaporation were constant, we should be able to deduce, from the indications of the urinometer, the exact proportion of water; and by overlooking this source of error, Becquerel has drawn up a table for this purpose. It must be premised that the temperature at which the densities are calculated is 50°.

1100.....	1000	0.0
1002.....	996.7.....	3.3
1004.....	993.4.....	6.6
1006.....	990.1.....	9.9
1008.....	986.8.....	13.2
1000.....	983.5.....	16.5
1012.....	980.2.....	19.8
1014.....	976.9.....	23.1
1016.....	973.6.....	26.4
1018.....	970.3.....	29.7
1020.....	967.0.....	33.0
1022.....	963.7.....	36.3
1024.....	960.4.....	39.6
1026.....	957.1.....	49.9
1028.....	952.8.....	46.2
1030.....	950.5.....	49.5
1032.....	947.2.....	52.8

It is almost unnecessary to remark that this table is constructed upon the supposition that there is an increase of 1.65 in the solid contents, for each degree upon the scale of the urinometer.

For ordinary practical purposes it is perhaps accurate enough, but even at the highest temperature obtained from a well-regulated water-bath, a slight decomposition (probably of the organic matter) seems to occur. Dr. Percy, in his admirable papers upon diabetes recently published in the "Medical Gazette," has shown that a difference, amounting to upwards of five grains, occurs between the weight of the residue of 1000 grains of urine evaporated over the water-bath, and that evaporated over strong sulphuric acid in vacuo.

Upon the whole, I am inclined to think Becquerel's table tolerably correct, the error (if any) being from over-evaporation. The statements of chemists upon this point are extremely discordant. Berzelius makes the solid residue of 1000 parts of urine at spec. grav. 1029 amount to 67 parts, and Lehmann obtains from 1000 parts of urine at 1022, 65.825 parts of solid residue (nearly double the quantity assigned by Becquerel's table).

According to Vogel, the minimum of solids in 1000 of urine was found to be 12.5; the maximum 71.5. The mean of 40 analyses gave 40.0 in the 1000 of solids. This corresponds very nearly with that which is assigned to sp. gr. 1024 in Becquerel's table, a very frequent density.

Urea.—The discovery of urea is ascribed by most French chemists to Rouelle, who certainly described something under the title of *saponaceous extract of urine*, of which urea must have been the principal ingredient. This is obvious from his observation regarding its capability of being converted by heat into carbonate of ammonia. Our own countryman, Cruickshank, seems to have been the first person who obtained it in an isolated form; he also discovered i property of crystallising with nitric acid. The sub-

Regarding its general properties in a state of purity, we may remark that when its watery solution is quickly cooled, and crystallisation rapidly induced, it assumes the form of white, glistening, silky needles; upon slowly cooling it crystallises in colourless, flat, four-sided prisms.* Its specific

* Handbuch der Medizinischen Chemie. Berlin, 1840-2.

† The subdivisions of "extractive matter" will be noticed in a subsequent part of these papers.

* In a solution of common salt it crystallises in octahedrons, and in a solution of muriate of ammonia in cubes. Upon this pro-

gravity is 1.35; it is of a coolish, salpetre-like taste, and free from any smell; it is soluble in its own weight of water at 60°; and much more soluble in hot water. From four to five parts of cold, and two (or rather less than two) parts of hot alcohol dissolve it. Ether and the essential oils take up very little urea. According to Dr. Percy, one part of pure urea dissolves in sixty-three parts of common rectified ether (of sp. gr. 759.3 at 59° F.) at 49° F.

Various methods have been proposed for separating the urea from the urine. The following is one of the best; in all essential steps it corresponds with the plan described by Lecanu:—

Evaporate four ounces of urine over a water-bath until the fluid assumes a thick syrupy appearance, that is to say, until it is reduced to about one-sixth or seventh of its original bulk. Add to this about two ounces of alcohol. Apply a gentle heat, and filter. The filtered alcoholic solution contains the urea, colouring matter, a certain quantity of animal extract, lactic acid, and the lactates; while the alkaline and earthy salts, as also the uric acid and the urates, are precipitated. Wash the filtering paper and its contents with a little more alcohol, in order to take up any urea that may have been left; mix the two alcoholic solutions, and evaporate them over the water-bath to a syrup, and add an equal volume of pure nitric acid previously diluted with its own bulk of water. Upon stirring up this mixture a crystalline mass is at once formed (consisting of nitrate of urea), containing in its interstices a certain quantity of thick syrupy fluid, consisting of lactic acid, organic matter, &c., which must be removed by pressure between the folds of a piece of very fine linen or calico. Any excess of nitric acid or animal matter that may be still entangled in the crystalline mass, must be removed by plunging the linen containing the nitrate into water at 32°, and strongly compressing it. It then only remains for us to press the nitrate of urea between some leaves of blotting paper, and to dry it at a very moderate heat. We can then readily calculate the quantity of urea from the weight of the nitrate. The crystallised nitrate of urea contains one atom of water of crystallisation.

In 100 parts of the nitrate of urea we have,

Urea	48.938
Nitric acid	43.781
Water	7.281

Lecanu, from omitting to notice the water of crystallisation, assigns the following proportions to this compound:—

Urea	53.07
Nitrtc acid	46.93

With respect to the general properties of the nitrate of urea it is sufficient, in the present place, to state that it crystallises in large brilliant plates (the size of the plates apparently varying inversely with the rapidity of the cooling), that it is soluble in alcohol, but more so in water.* If heated rapidly, it explodes, being converted into nitrate of ammonia.

If it were required to obtain the urea in an independent state, it might be done by neutralising a solution of the nitrate with carbonate of potash, and concentrating the mixture. The nitrate of potash crystallises first, and the urea afterwards, from the mother liquid of those crystals.

The chemical and physiological relations of urea will form the subject of a separate paper. We proceed, therefore, to the consideration of uric acid.

In order to embrace in a single paper the principal details of the *analysis of the urine*, we shall defer to a subsequent occasion the consideration of various disputed points regarding this substance, and merely point out at present the best method of separating it from the urine. We must premise that it is insoluble in alcohol and in ether, and only slightly soluble in water.†

The best method for collecting the uric acid in ordinary analyses is the following, insomuch as we are by this means able to separate the uric acid and the urea from the same portion of urine. The precipitate which we described as being left on the filter must be washed with a little dilute muriatic acid, which has the property of dissolving all the alkaline and earthy salts, of reducing the urates (if any happen to be present) to uric acid, and of acting upon a little of the animal extract. Upon filtering we obtain a deposit of a greyish-white colour, consisting of very nearly pure uric acid. It then only remains for us to dry and weigh it.

Becquerel observes that during the process a low temperature ought to be observed; without this precaution warm muriatic acid

perty a very good microscopic test for the presence of minute quantities of urea in the various animal fluids, &c., has been founded.

* Nitrate of urea is soluble in 8 parts of cold water. Graham's Chemistry, p. 995.

† From Dr. Prout's experiments it appears that uric acid requires at least 10,000 times its weight of water at 60° to dissolve it. Berzelius and Mitscherlich quote his statement, and appear to consider it correct. Berzelius accounts for Henry's statement that it is soluble in 1720 parts of cold and 1400 parts of boiling water, by supposing that he experimented upon a sub-urate of ammonia instead of pure uric acid. Lehmann states that it is soluble in from 1800 to 2000 parts of cold, and in from 500 to 900 of warm water. The subject requires further investigation.

would dissolve a portion of the uric acid, which would not be precipitated on cooling.

The Inorganic Acids and their Salts.—The method which I usually employ in estimating the proportion of these substances applies only to those which can resist the action of a red heat. It was first described by Lecanu, and is founded upon the principle of the conversion of urea into nitrate of ammonia through the agency of nitric acid and a high temperature. To four ounces of urine evaporated to a thick syrup, add about two drachms of pure nitric acid. Continue to apply heat, until the mixture is reduced to such a state as to become solid upon cooling. When it is quite cold (and consequently quite solid), project it in small quantities at a time into a platinum crucible kept at a red heat. Sudden deflagration occurs; the organic matters are almost immediately destroyed, and the ammonia-salts dissipated, and there is left a residue of the fixed salts, forming a hard white mass at the bottom of the crucible. After continuing the heat for a very few minutes remove it from the crucible, and treat it with an excess of distilled water acidulated with muriatic acid.* When the saline mass is perfectly dissolved add nitrate of baryta; a mixed precipitate of the sulphate and phosphate of baryta immediately falls. By the addition of nitric acid, the phosphate is dissolved, and from the weight of the dried precipitate of sulphate of baryta, the quantity of sulphuric acid may be readily estimated.

In order to estimate the quantities of phosphoric acid and chlorine we must pursue the following method:—The acid solution (or a known portion of it) must be neutralised with ammonia, and then boiled, in order to expel any excess of ammonia that might have been accidentally added. Upon the the addition of a solution of nitrate of silver a precipitate is thrown down, consisting of the phosphate of silver and chloride of silver. Dry and weigh this precipitate, and treat it with nitric acid, which dissolves the phosphate and leaves the chloride of silver, which must be dried and weighed, and from which the quantity of chlorine may be estimated. The quantity of phosphoric acid may be estimated from the weight of the phosphate of silver, which is known by subtracting the weight of the chloride from that of the mixed chloride and phosphate. The potash may be determined by the usual test of the chloride of platinum, and calculated accordingly.

If now we substract from the weight of the saline mass the sum of the weight of the sulphuric and phosphoric acids, of the chlorine, and of the potash, we shall have, as a remainder, the aggregate of the weight of the soda, lime, and magnesia. If we wish to carry this analysis still farther, we may add an alkaline carbonate to the solution of these substances, which will throw down the magnesia and lime in the form of carbonates. These may, again, be separated by means of chlorate of potash, according to the method recently proposed by Döbereiner. For general purposes it is usually quite sufficient to record the weight of the fixed salts without going through the somewhat tedious process of separating them from one another.

We have thus pointed out the method of ascertaining the relative quantities of the water, urea, uric acid, and fixed salts, in a given quantity of urine. The difference between the sum of the weight of these *determined* substances and the weight of the urine, represents the weight of our 5th and last class of the tabular form given in an early part of this paper, viz., "extractive matter, lactic acid, and certain lactates, mucus, colouring matter, and those salts which cannot resist the action of a red heat,"

Southwick-street, Hyde-park-square,
May, 29, 1843.

(To be continued.)

* The saline mass is always found to be alkaline in consequence of the destruction of the lactic, and the dissipation of the carbonic acid, and the consequent freedom of a portion of soda, and perhaps of lime.

STRYCHNIA.—The "Gazette des Hôpitaux" of April 25, reports the case of a man, twenty years of age, who, after suffering from gastro-intestinal irritation for a considerable period, was attacked by paralysis of the lower extremities, with diarrhœa, and excretion of urine, amounting to a quantity three times as great as that of the liquid drunk (yet not containing sugar.) Nitrate of strychnia was given in doses of 1-10th grain every six hours. In the course of four days some motion of the limbs returned; and by the time six grains of strychnia had been taken, both the paralysis and diabetes had disappeared.

THE TRUTH WILL OUT.—During the great continental struggle early in the present century, and while the French held sway in Austria, a young diplomatist was sent from Paris to Vienna, charged, among other government missions, with a sum of gold which, for greater security, he carried in a girdle beneath his other clothing. On reaching the term of his destination, and unburdening himself of his weighty charge, what was his astonishment to find his money white! He wrote back to Paris stating that silver had been given him instead of gold. A government inquiry was at once set on foot, in the progress of which it was elicited that the diplomatist had been previously under a long mercurial course for a venereal complaint, a piece of intelligence that but for the unlucky affinity of mercury for gold would doubtless have remained buried in oblivion.

HAIRS UPON THE TONGUE.

To the Editor of THE LANCET.

SIR,—In your Journal of last week, p. 359, I perceive a notice on the growth of hairs upon the tongue, quoted under the authority of the "Oesterreichische Medicinische Wochenschrift," and referred to by you as a statement unworthy of credit.

On the continent the belief in the occasional presence of hairs upon the tongue is by no means uncommon, and has for its supporters several distinguished names. Bichât in his "General Anatomy," enumerates, among the situations in which hairs are sometimes accidentally developed, the surfaces of mucous membranes, as of the stomach, the intestines, the bladder, and the gall-bladder. On the mucous lining of the latter organ he found on one occasion about a dozen hairs, implanted by their roots into the tissue of the membrane. Villermé, again, announces that hairs have been found on the tongue, and in the pharynx, the rectum, the uterus, and the vagina.

If we turn our attention to the structure of the mucous membrane we find that it is not organised for the production of hairs, and in this particular differs essentially from the external investment of the body, which, in every point of its surface, is endowed with hair-producing follicles. It would, perhaps, considering the close analogy subsisting between the mucous membrane and skin, be presumptuous to declare that the production of hairs by the mucous membrane is impossible; but we may safely affirm that such an event is highly improbable. Admitting these premises, we have no alternative but that of coming to the conclusion that Bichât and the other authorities on this subject have been deceived by the appearance of a something resembling hairs. And I think this the more likely to have been the case from the imperfect knowledge of the structure of the skin existing in the time of Bichât, and from the absence of the only satisfactory test in a case of this nature, the microscope. Indeed, there is sufficient reason to believe that at the period referred to, such a definition as the following would have been amply characteristic of a hair:—*A filament, hair-like in form and dimensions, cylindrical, uniform in diameter, adherent by one extremity to the surface of an organ;* but your youngest reader must be aware that at the present day better proof is needed than such a definition could embrace.

With the kind of case described by the Austrian periodical, I am very familiar, having had my attention drawn to the subject during some microscopic investigations, about seven years since, and having subsequently seen many similar cases. In a work published in 1838, I have referred to these hair-like tufts growing on the tongue, and I have there remarked, in respect of them, that "the cuticular sheaths" of the papillæ of the tongue "become enormously enlarged and lengthened in some fevers."* This is the real nature of these productions: they are epithelial sheaths of papillæ, grown to a length of half or three-quarters of an inch, cylindrical in form, and hair-like in appearance and general characters; indeed, without the aid of the microscope and recent knowledge on the mode of development of the epithelium, we might easily believe them to be hairs. About a year after my first observation of these filamentary sheaths, I had the opportunity of conversing on the subject with Henlé, who quite agreed in the views which I had taken of them, and mentioned that he had seen a similar appearance in the epithelium of the alimentary canal.

This explanation applies with equal force to the other mucous surfaces indicated by Bichât and Villermé, and is peculiarly suited to the case narrated in the Austrian hebdomadal, for as we there read the so called hairs "disappeared under the influence of an emetic," a phenomenon not at all consistent with the kind of connection maintained between hairs and their papillæ, but quite in unison with the supposition of their being *elongated epithelial sheaths.* I am, Sir, your obedient servant,

ERASMUS WILSON.

London, June 5, 1843.

QUININE IN TRAUMATIC FEVER.

A CASE has lately occurred in the Hôpital des Enfans at Paris, in which M. Guersant has employed, with entire success, the sulphate of quinine in the treatment after amputation of both lower extremities. The patient, a poor boy, whose limbs had been frozen by exposure during a winter's night, and afterwards most injudiciously bathed in hot water, was brought to the hospital, the limb exhibiting a discoloured spotty appearance, and other symptoms of incipient gangrene. This condition in six days, notwithstanding the application of bark-poultices and bottles of hot water to the extremities, so greatly increased as to render necessary an immediate amputation of both legs. The operation was successfully performed, and with little suffering to the patient, who gradually recovered, though not without indications of considerable fever and erysipelatous inflammation, both of which were subdued by doses of sulphate of quinine, amounting to twelve grains in a day. M. Guersant cites other cases, also in which he has employed the same medicine with success, in all of which the leading feature was purulent reabsorption—*Gazette des Hôpitaux*, March 14.

* Practical and Surgical Anatomy, page 307.

FEVER,
ITS
NATURE, CAUSES, & TREATMENT,
THEORETICALLY AND PRACTICALLY CONSIDERED; IN CONTINUATION OF A PAPER ON
CHOLERA.

By C. Searle, M.D., M.R.C.S.L.

Having, in a former paper, represented the connection as most intimate between cholera and fever, I have now to bring under consideration the distinctive character of each. This essay will necessarily embrace a theory of fever, which I submit to the profession with some confidence, as I think it affords an explanation of this affection of the system under every circumstance and condition, in addition to its being a fair deduction from the various experiments of Dr. Philip and others on this subject and inflammation; and more than this, I trust that it will clear up most of the difficulties which at present exist with respect to the treatment of fever in general.

Cholera I have represented to be a disease marked by the subduction or prostration of power, or in which symptoms of excitement are but seldom evinced beyond such as result from topical inflammation. Whereas, the characteristics of fever in general may be said to be the very reverse of this,—viz., increased action of the heart and vessels of circulation, and general excitement. These effects may result, either as a consequence of the direct operation of a stimulus on the heart and vessels of circulation, or from the blood abounding or being in excess of inflammable particles; or else the excitement of the powers of the circulation may be indirectly and secondarily induced. Hence the class Pyrexiæ is divisible into two orders, the synochal, and the adynamic or typhoidal. To the latter, however, as being the subject, from its connection with cholera, of our more particular inquiry, we shall principally confine ourselves, the former, indeed, requiring no elucidation to render obvious. Illustrating the subject by the operation upon the system of cold, the most frequent of all the ordinary causes of fever; the sedative agency of which producing effects, as we shall shortly make appear, analogous to the contamination of the blood. The fever, however, originating in this cause may be of either order, according to the conjoint operation of circumstances or state of the system at the time of attack, as we shall hereafter more particularly notice.

By cold, or the undue abstraction of the stimulus of heat from the cutaneous surface, the functions of its capillary vessels in the extrication of carbon and absorption of oxygen, as well as of its elimination of the perspirable exhalation, are rendered torpid or arrested; and in consequence of the diminished absorption of oxygen and exhalation of carbon, chemical action and the organic capillary function throughout the system is diminished. A diminution of the heart's action necessarily ensues, in common with debility of the rest of the functions, and accompanied with sense of chill and depression o the system, as we have previously explained in treating of the development by malaria of the symptoms of cholera. To the extent alone, however, in the present case, of its inducing that degree of congestion in the veins which eventually brings about reaction in the arteries; or, in other words, the engorgement of the veins that takes place, impeding the transmission of blood into them from the arterial capillaries, distention of these vessels and of their trunks ensues, and by consequence, their excitement, or increased action, follows. And this effect being general throughout the system, and, therefore, appertaining to every organ and structure, the heart and brain are necessarily involved; and hence follow their excitement and general fever, in contradistinction to inflammation, which is the same in character, but limited in the extent of its operation, to the particular organ or structure affected, —the seat of its occurrence.

Let it, however, be distinctly noticed that I do not mean to confine the operation of cold to the cutaneous surface, as the pulmonary, of necessity, must often be simultaneously thus implicated; and it may be and often is, extended to the general system, influencing it at once, and thus directly affecting chemical action and the function of the capillaries. But its effects on the cutaneous surface I have placed in the most prominent position, as these include all that can be said in explanation of the general influence of cold upon the system. The suppression of the perspirable secretion and exhalations from the skin may, by the retention of their elements in the blood, prove an addition to the amount of causes of derangement, and a modifying circumstance, by which rheumatism in some, and diarrhœa in other cases, or other disorders, may be superadded to the febrile affection, as cough and bronchial irritation succeed to the effects of cold on the mucous membrane of the air-passages.

The excitement of the capillaries by congestion in the veins, I am of opinion, is brought about in the following way:—The engorgement, which takes place at the extreme points of the circulation, withdrawing so much blood from the current returning to the heart, less blood is transmitted to the lungs, and proportionately more air (oxygen) is received into it from their surface,—seeing that it has been determined by the experiments of Magendie and others that absorption from all the surfaces takes place in the inverse ratio with the distention of the

blood-vessels; and it was proved that absorption from the lungs was more energetic than from any other surface of the body,—a fact for which I have offered a sufficient reason in my former communication. More oxygen being now received into the system, increased chemical action and its consequence, capillary excitement, become developed, to the removal of the obstruction. Hence the reaction of fever is not the disease, but a curative effort of the system; and thus do surgeons of the present day, in all cases of injury, wisely delay bleeding till reaction becomes developed.

There is another mode, too, by which capillary excitement is, as I am of opinion, brought about; but this, perhaps, more particularly obtains in local congestions developing inflammation. I would not, however, limit its operation to this state alone, as it most probably aids in the development of fever also. I have, in a former paper, adduced the experiments of Mr. Coleman and Sir A. Cooper, in testimony of the fact, that although venous blood is in the first instance found to be some degrees hotter than arterial, arterial blood eventually becomes hotter than venous, by from three to six degrees. Now, this progressive increase in the temperature of arterial blood establishes, in my mind, what I have asserted to be the case in opposition to Liebig, that the combustion of the carbon and hydrogen of the blood is not confined to the capillary system, but is progressive in the system from the moment oxygen is received into the blood, when chemical affinity and combination of the inflammable elements of the blood immediately commence; and thus is it that venous blood attains to the maximum of temperature: a wise provision for maintaining the temperature of the lungs, to which venous blood is furnished, and for precluding the necessity of any chemical change being effected in the blood in these organs for the purpose, which would otherwise be effected at a great disadvantage or sacrifice of result. The changes effected in the blood in the lungs are, therefore, I maintain, purely of a physical character, consisting of exhalation and absorption simply.

As respects my subject,—the excitement of increased capillary action by venous engorgement, the latter, by retarding the passage of the blood through the capillary vessels, would necessarily occasion arterial distention and a preternatural development of heat and electricity in these vessels, and, in consequence, undue excitement of the arteries, seeing that the chemical changes in the blood and development of heat are progressive. Hence, the increased temperature and sensibility, the throbbing and swelling of a part in inflammation.

In fever, excitement of the capillaries taking place, synochus or an inflammatory type will be the consequence, whenevert ne condition of the blood or system is favourable to this form of affection—that is, in persons who previously were in health, and especially those of phlogistic habit, or, in other words, whose blood abounds in combustible particles, or whose susceptibilities are great. On the other hand, the type will be adynamic or typhoidal, whenever the blood is impoverished, or from contamination with malaria or other sedative agency, is favourable to this condition,* in which the fever, having certain periods of exacerbation and remission, may be said to be uniformly of an intermittent or remittent character. So that the cause of excitement being the congestion in the veins, whenever this condition is removed by the excitement developed, the excitement ceases; but this, again, necessarily recurs with the congestion which ensues afresh, as in the first instance, if the cause which gave rise to it be not removed in the interim, or its effects obviated. That is, so long as the system retains sufficient power, or, in other words, the capillaries adequate susceptibility to impression, but which, sooner or later, must be exhausted by this untoward action, and the complications which ensue; and hence it is, from the debility that ensues upon the continuance of the disease, that the quotidian succeeds to the tertian, and the remittent to the quotidian type of fever, and that also the adynamic so closely follows upon the heels of the synochal order.

Superadded, however, to this idiopathic fever, in proportion to the severity of the cause, susceptibility of structure, or condition of parts, and state of the patient with respect to fulness and condition of the blood, will topical engorgement or inflammation be concomitant; which congestion or inflammation will in general be seated in those parts of the system the most remote from the heart's influence, and assigned to them in cholera; which being in the stomach and bowels more particularly, and also in the head, explains how both Clutterbuck and Broussais were respectively led to the mistake of assigning to these organs the exclusive seat of fever.

Reverting to cold, we have analogical illustration of this cause of excitement, in the operation of other sedative agents upon the system. Opium may be mentioned,

* Hence marsh miasma, the proximate cause of intermittent fever, and exposure to cold often an exciting one, even at a considerable distance of time from the period of inhalation. It must, however, be obvious that any other debilitating cause may be productive of the same effect as cold in the instance adverted to; hence the exciting causes of fever in general are, as we find them to be, debilitating agencies of every kind.

small doses of which produce, primarily, a certain degree of excitement; and the same effect follows a less degree of cold than is productive of fever, as in the cold bath; whereas a large dose of opium induces an opposite effect, or, at least, the excitement, if it really produce any, is so transitory that it cannot be calculated upon; and this is followed, if the dose be poisonous, with overwhelming cerebral congestion: which is exactly what takes place in the severer attacks of cholera, and holds equally good in the continued exposure to the severer degrees of cold by which the respiratory function is arrested, and sleep and death are occasioned.

There is another cause of fever, however, which has been treated of by Dr. Marshall Hall, and well merits attention, most forcibly illustrating the operation of sedative agents and debilitating causes in general in the production of excitement, connecting the whole into a chain of reasoning, which appears to me irresistibly conclusive on this head. This is the reaction following hæmorrhage or excessive blood-letting; the loss of an undue quantity of blood being productive of general torpor or debility in the first instance, which is followed by excitement, and is succeeded by exhaustion or collapse; the same progression of symptoms exactly which takes place or succeeds to the other debilitating causes of fever, and which may be thus explained:—From the loss of blood sustained, the quantity returned to the heart not being in quantity sufficient on contraction of the left ventricle to distend the aorta, little is carried to the brain, inasmuch that a certain degree, if not full distention of the aorta, would appear essential to its ascent in the carotids, in opposition to its gravity, when we consider that the descent from the arch of the aorta must so materially favour its progression in that direction. And as a further consequence of this want of distention in the aorta, a comparatively small quantity of blood would be circulated through the coronary vessels of the heart also; and hence the organic structure of both heart and brain not being duly excited, torpor of their functions and debility or syncope ensues. This is necessarily followed, as previously explained, by venous congestion, and the latter by excitement; provided the system retains power sufficient, or the capillaries susceptibility enough to impression, but which necessarily becomes exhausted by preternaturally increased action; hence follows collapse, as mortification succeeds to inflammation. The same views are equally well supported by the operation of the opposite class of agents on the system,—heat in moderation producing excitement; excess of heat, debility; and extreme heat, as by burning or scalding, mortification and death; wine or alcohol, also, in reference to quantity, producing exhilaration, atony, or death;—rendering the whole a mass of evidence which appears to me irresistibly conclusive, and converting what would otherwise be mere hypothesis into a satisfactory theory.

Founded upon the foregoing explanation, fever may be defined—a state of system under general capillary excitement; and hence the usual indications of its presence are increased temperature of the body and velocity of the pulse, effects, however (we have attempted to make appear), which may arise either from the direct operation of a stimulus upon the powers of circulation, or that may be indirectly and secondarily induced; the former giving rise to the synochal and the latter to the typhoidal order of fevers.

In thus viewing fever, what are the indications of cure? Assuredly not the subduing of excitement simply, or the symptoms of inflammation exclusively should these even be more particularly manifest; as the former I think I have rendered pretty clear are but effects of direct curative tendency, and the latter are also developed by the same agency. Notwithstanding, such is the advice of many talented writers upon this subject, which, with the too prevailing doctrines of Broussais and Clutterbuck, directing us to look upon fever as in all cases symptomatic of inflammation, founded upon post-mortem examination, are daily and hourly most destructive in their consequences, and call for decided refutation.

No; febrile excitement is but an effect, and in the majority of cases—the typhoidal order being, doubtless, the more frequent forms of fever—an effect of a morbid or ill condition of the blood; and is not only an effect, but I believe, also, a curative one, which, however proper it may be to moderate, or keep within due bounds, it is clear it should not be the chief indication of practice to subdue; our chief aim and object being, as in other cases of disease, rather directed to the cause or condition of system giving rise to it; and as this may depend upon, or consist in, repletion of the vessels, or the over-stimulating quality of the blood on the one hand, or, on the other, the reverse of these, the blood being deficient either in quantity or quality with respect to stimulation, from inanition or hæmorrhage, in the first instance, or from contamination with a sedative agent, or impoverishment in the second, the treatment must vary accordingly. In the first case of inflammatory fever, in reference to both cause and effect, blood-letting is of direct curative application, and, in conjunction with means of the same indication, depletory and sedative, is found to be the most successful practice. But however useful the practice of blood-letting may be in the foregoing condition, it is a remedy of but secondary indication in the treatment of the second or typhoidal forms of fever.

Though, doubtless, a remedy capable of fulfilling certain indications of curative tendency in these also, judiciously employed; it is certainly anything but the remedy of chief or paramount importance, this rather bearing reference to the cause or causes remote or immediate, predisposing or exciting, giving rise to it.

This calls upon me, before proceeding further, for another observation, viz., that the type of fever and grade of malignancy are modified to a considerable extent, not only by the constitutional state of the individual at the time of attack, but by the nature of the exciting cause,—the primary or remote, in the case of miasmal inhalation, being, I believe, seldom of sufficient concentration to the development of fever without the aid of the latter. Hence it is, as I have previously noticed, that some persons escape the attack of intermittent fever till some time after removal from the source of the deleterious emanation, when some accessory agency or exciting cause, as exposure to cold, or other debilitating effect, developes its action.

It will be remembered that I defined malaria, as a cause of cholera, to be the gaseous production of organised substances, both animal and vegetable, in a state of decomposition; and as a cause of fever I would not limit the application of the word to the gases resulting from decomposition, being of opinion that other exhalations and causes of deterioration of the atmosphere, whether connected or not with the decomposition of organic remains, may be productive of the same effect. For instance, a number of persons inhaling a confined atmosphere, and particularly such an one as must be respired by the inhabitants of filthy, damp, unventilated cellars, or in the hold, or lower decks on ship-board; independent of the exhalations ensuing from the decomposition of sapulent wood and other organic substances in the latter case, and that these, particularly the first mentioned, are not unfrequent causes of fever, is unquestionable; and in further illustration may be mentioned, the fact recorded of Mr. Howell's party, thrust into the black-hole of Calcutta, the survivors of which were, it is said, immediately attacked with typhus, and carbuncles, as in plague; and from the like condition of atmosphere taking place about the persons of the sick, in dirty unventilated apartments, is it that contagious *fomites* become elicited in such situations. Hence, likewise, to the production of the severer species of typhus we may infer, the conjoint operation of the greatest combination of the requisites to that end, or the remote cause in the highest degree of concentration; and thus it is that the miserable, half-starved, and too commonly depraved inhabitants of filthy, damp, unventilated cellars, or the like description of cabins in Ireland, are the most frequent subjects of such attacks; and all the grades between thisform and intermittent fever, the mildest species of the order, are but modifications resulting from the more or less favourable combination of causes to the production of fever and state of the system to its development. Hence a severe rigor marks active conservative energies, and by consequence, as I know by experience in intermittent fever, a less severe form of affection, or rather of a more healthy condition of system; and hence the foundation, perhaps, of the antiquated notion, that a spring ague was wholesome.

Bath, March 29, 1843.

(*To be continued.*)

TYPHUS IN ANIMALS.

M. Rayer opened the body of an ass's foal, six weeks old, which had died after an attack of diarrhœa, lasting for a week. The morbid appearances were similar to those seen in the human subject dying during the first stage of typhus fever. The lesion most distinctive of this disease is an inflammation of the glandular follicles of the small intestine (*fievre entero-mesenterique*). In the solipede animals the Peyerian glands are indeed highly developed, even in a state of health; but in the instance in question they were not only extremely salient throughout the internal surface of the intestine, but in certain places they had a deep red colour, one group of them being partially ulcerated. The whole internal surface of the small intestine, but especially its upper portion, was greatly injected. It was lined by no coating of plastic lymph, such as is seen in dysentery, but it was filled with a liquid which, in some parts of the tube, had a greyish and in other parts a rosy tinge. The cæcum and upper part of the colon more or less presented similar appearances; the lower part of the large intestine was healthy, as were the liver, kidneys, bladder, thoracic viscera, and brain. The mesenteric glands were swollen; some were reddened and variegated with dark red streaks, others were so injected with blood as to have their colour altogether changed to a dark brown. A large ecchymosis appeared at the pyloric extremity of the stomach; the spleen, of a greyish-brown colour, was neither softened nor perceptibly enlarged.

The writers on veterinary pathology have noticed in the typhoid affections of cattle a diffused inflammation and ecchymosis throughout the intestinal canal, a swollen state and softening of the spleen, *ramollissement* of the lungs, &c., but they have not hitherto recorded the diseased state of the glands of Peyer, and those of the lacteal apparatus, which was obvious in the instance quoted above, and is certainly the most decided pathognomonic circumstance attending typhus in the human subject.—*L'Experience.*

CASE OF

PSORIASIS PALMARIA,

IN WHICH THE

ENTIRE LIVING BODY BECOMES LUMINOUS

FROM A

PHOSPHORESCENT SECRETION.

To the Editor of THE LANCET.

SIR,—I recollect reading in your Journal some years ago of a human body luminous after death. I offer to place on record the same extraordinary phenomenon in a living patient. Mr. S., ætat. 43, of tall stature, robust, of Herculean strength, active, capable of great mental and bodily exertion, has been for some time annoyed with psoriasis of the palms. He is a gross feeder, extremely fond of the fat of meat, in the use of which he indulges very extensively. I have known him a quarter of a century, and do not recollect to have seen any man of sedentary occupation habitually consuming so large a quantity of adipose food. Towards the end of this last winter he had severe catarrh, which brought on an extensive bronchial expectoration; but on his return from a tour of business on the continent this had entirely disappeared, his hands, however, becoming unusually troublesome. For several days prior to his skin becoming phosphorescent he had been indulging in the use of grossly fat food to an extent exceeding his usual habit, and had been taking large quantities of lemon-juice, his own remedy for psoriasis. I may here add, that he is very temperate in the use of fermented liquors, and seldom takes spirits. He had been reading late, as he is wont to do, and on retiring he had clothed the head of a chair with his shirt instead of throwing it over the chair-back. On getting into bed he saw to his surprise a blaze or halo of phosphorescent flame, representing the headless trunk of a human body, at the foot of the bed, where he had disposed his apparel. His curiosity being excited he got out of bed, and approaching and handling the shirt, he was induced to examine his own skin. He found his arms and his chest in the same luminous state. When the phosphorescence faded on the linen friction restored it. The phenomenon continued for several days, and has since recurred under the same error of diet. My patient is accessible to a further curious investigation of this interesting, and I believe, rare appearance, for although he is a disciple of Moliére he is a scientific man.

I shall not trespass on your readers by offering a lengthened explanation of such an extraordinary secretion; that in this case the phosphorus is not duly united with oxygen is evident, and therefore the pathological condition would seem to be an imperfect intra-combustion. Phosphoric acid is formed in the human body, and healthily eliminated through the ordinary channels, the kidneys, skin, intestinal canal, and in the female by menstruation. I can recognise that the defect leading to the phenomenon is the imperfect or loose combination of the phosphorus with oxygen. *The fact* justifies *the reasoning*, but the *reasoning* does not explain the *fact*, and that is precisely the condition of the aggregate facts and reasonings which constitute *animal chemistry*, so called.

Indulge me one moment, though I walk on the hot ashes of intra-combustion. *Chemistry.*—What is it? Part of the code inanimate. Physiology? The code animate. But what is *animal* chemistry? The code animate-inanimate? The phrase is utter nonsense. It is a solecism. As well might you talk of *animal mechanics*, for life is a state which holds the mastery over both codes; that is to say, the chemical and mechanical, and that is the most perfect condition of life in which such pre-eminence is most perfect. If too great or too languid then comes disease. This compound term is the cockney cant of English-Havannah's transferred to the language of science. If I use the term in publishing a series of facts and reasonings it is for the sake of conformity only. French law? Yes. English-law? Yes. English-French law? No. Who can imagine a nation governed by two codes of laws, antagonising each other, unless we presuppose a *tertium quid* harmonising the whole. The living principle is that *tertium quid*, and until we can arrive at a knowledge of it, the reciprocal laws of animate and inanimate bodies, like the international laws of the world, will remain vague and obscure. Yours obediently,

G. F. COLLIER.

Spring-gardens, June 1, 1843.

P. S.—If we admit the term *animal chemistry*, why not vegetable chemistry, animal mechanics, vegetable mechanics. If the laws of gravitation are more than healthily antagonised, as in catalepsy, or less, as in typhus, and other diseases of debility, do we not see that the same sort of balance of codes is essential to health? If we go on with these compound terms we shall next have animal arithmetic and vegetable arithmetic, for nature is a great arithmetician as she is a great chemist. In truth, animal chemistry is nothing but physiology.

THE ODDS AGAINST HIM.—Corvisart was deploring the death of Dr. B. "However (said he) it is a consolation to know that he was not sacrificed to want of attention. During his last days Hallé, Portal, and myself never left him!" "Alas!" said the abbe Sièyes, "what would you have had him do against three?"

DOES THE LIVING LEECH PRESERVE THE LIVING POWER OF THE BLOOD?

It has been supposed by some authors that the body of the leech has the power of keeping the blood from coagulating by keeping it (so to speak) alive. As I had not made any observations myself on this subject when I published my work on the blood, I shall here state a few experiments which I have made lately.

Experiment 1.—About twelve or fourteen hours after a leech had been applied, a little common salt was put on its body, the blood vomited was thickish, and remained in the same state without further change. It did not coagulate.

Experiment 2.—As the leech seemed to suffer considerably from the application of the salt, and as it was difficult to prevent this touching the blood, and thereby tending to keeping it fluid, a leech was suddenly cut in half, and the divided parts as quickly thrown in boiling water, after it had been only from *two to three hours* applied. The blood was florid and perfectly fluid, and *when examined an hour afterwards remained in the same state.* Also when examined twenty-four hours afterwards.

Experiment 2 was repeated with a like result.

The result, in two very careful experiments, was likewise the same when the leech was only left an hour to an hour and a half after having been on the body.

These experiments show, satisfactorily, why the late Mr. Thackrah found no trace of coagulation after blood had been in the body of the leech an hour.* But they also refute, satisfactorily, his opinion that in the case of the leech "the blood's fluidity is maintained by the life of its receptacles." In my work on the blood I have been inclined to agree with Hewson and Thackrah that the containing vessels have a degree of power in preventing coagulation. But I must now confess that all experiments previously made on this subject were unsatisfactory while it had not been ascertained that the blood remaining fluid would coagulate. Hewson, in admitting that blood enclosed between ligatures in the living animal remained fluid a quarter of an hour, still admits that he "found, on careful examination, sometimes two or three small particles about the size of a pin's head, which (he adds judiciously) are coagulated parts of the blood." In regarding specks in such light Hewson showed his judgment; and it seems far more probable that this blood had coagulated to this extent before examination was made than after, *in consequence of exposure to air*, as Thackrah suggests. My reason for such opinion is that I always found more or less of such specks when blood was kept permanently fluid by exposure to a heat of 140°, as the reader will find in the experiments detailed in my work just referred to. Such specks in the case of heat of 140°, and hence probably (when not ascertained to the contrary *by observing if such fluid blood will coagulate*) in all cases, are to be considered the coagulation of the so-called fluid blood. I have had repeated occasion, in the work referred to, to allude to the very loose way in which even some of the best experimentalists operated on the blood, in not distinguishing between coagulation and consolidation. The experiments just related on the leech still further convince me of the same fact, since we find Thackrah, in this case and in numerous others, where he put a ligature on veins, altogether omitting to ascertain whether the fluid blood had or had not lost its coagulating power, and whether the coats of such veins did not permit the passage of air, as I, from late experiments on the difficulty of excluding air, am almost convinced they must do. Of course, until these points are ascertained, we must always be a little sceptical how far the contact of living vessels tends, *per se*, to keep the blood fluid,—fluid in the *correct sense of that term*—fluid and *yet capable of coagulation.**

On re-perusing Thackrah's numerous experiments on this subject, I do not perceive a single one to which these objections will not apply. In no case did he examine whether the fluid blood had or had not lost its power of coagulation. The same remark applies to Sir Astley Cooper's experiments on this subject. There is every reason to believe that a ligature on the veins suspends

* On the Blood, p. 82.

* I confess, however, that in the thirty-ninth and fortieth experiments Thackrah observed that the fluid blood did coagulate when let out of the heart of an ox. In the last experiment the animal had been dead "above half an hour." Yet "coagulation began in two minutes and thirty seconds." But these results, although favourable to the idea that in many of Thackrah's experiments the blood in the divided vein had not lost its power of coagulation, are wholly insufficient to prove the point. It is very doubtful, after an animal has been dead half an hour, whether the heart retains any irritability (life); for the involuntary muscles seem to lose their irritability sooner than the voluntary muscles. It is, therefore, quite as probable that the blood in the above cases was kept fluid by the exclusion of air (the temperature of the body not falling) as by the influence of the vital principle. Air was more likely to be well excluded in these cases than where flowing blood was received, by stop-cocks or otherwise, into dead veins or arteries, the very sides of which are, perhaps, often more pervious to air than those of living vessels.

the coagulation of the blood *for a time*, for we should not expect blood so circumstanced to coagulate so soon as if exposed to the air. But in the present state of our knowledge it may reasonably be presumed that Hewson's account of this matter is the correct one. Included between ligatures for ten minutes only it was quite fluid, and, I may add, in this time, probably had not lost its coagulating power; after fifteen minutes minute coagula, indicating that coagulation, as far as it could do so, had already taken place.

Thackrah's experiments, now reconsidered by me, seem only to show that in a lifeless vessel, as compared with a living one, the blood coagulates *ostensibly* and more firmly. The lifeless vessel seems to act as a stimulus, like any other extraneous matter, therefore the blood coagulates ostensibly in such circumstances. The great question, how long the blood, generally speaking, retains its coagulating power after having been inclosed between ligatures, is still open for future investigation. Sir C. Scudamore's experiments were not favourable to the idea of the living vessels having a power of retarding the coagulation of the blood. Nevertheless, Thackrah's experiments are not opposed to such idea, nay, they, on the whole, render the matter *probable;* but certainly they do not *prove* its correctness.

I shall conclude by observing that this subject may be much elucidated without any unnecessary cruelty on animals. Many experiments may be made on the vessels and hearts of sheep and oxen in a dying state, without seeking new subjects of experiment. But in all experiments on this subject it should be previously ascertained whether the blood-vessels of animals only just killed are not less pervious to air than those of animals killed the day before. Thackrah and others do not appear to have attended sufficiently to this source of fallacy.

HORATIO PRATER.

May, 1842.

FALSE CHEMICAL PREPARATIONS.—In the rage for the invention of new chemical preparations for medicinal use on the other side of the channel, many a "mare's nest" has been found, and combinations have been professedly discovered which subsequently turned out to have no real existence. We noticed that this was the case with the so-called hydro-ferrocyanate of quinia at p. 664 of our last volume. M. Mialhe has also proved that the hydro-sulphate of soda is nothing more than carbonate of that base with a little sulphuret of sodium and some other impurities, as proved by treating it with acetate or nitrate of lead; and that the pseudo-codeine which had got into use is actually arseniate of potassa.

POINTS IN MEDICO-LEGAL SCIENCE.

HOMICIDAL MONOMANIA AND MURDER BY PREMEDITATION.

AT a time when the subject of "homicidal monomania" has undergone so much discussion, the contrast of the following cases, one of which perhaps affords an instance of this species of insanity, is not destitute of interest:—

Mattos Lobo, twenty-six years of age, a member of a respectable family at Amieira in Portugal, had distinguished himself at college and was destined for the church; but *all on a sudden* he changed his mind, and went to Lisbon to attend the polytechnic school there. Among other houses which he visited, was that of his aunt, Donna da Costa, with whom lived a son, daughter, and female servant. As Lobo lived in a distant quarter of the city, he visited this family but seldom; but on the 23rd July last he called, and requested to be allowed to take his meals with his aunt until the 26th inst., when he proposed to leave Lisbon for the vacation—a request which was accorded. On the 25th, after having spent a part of the day in music with his cousin Julia, he went away saying that he should return on the next day and take leave. But on the same night about eleven o'clock a neighbour, who had noticed the outer door of Donna da Costa's house left open, mentioned the fact to the police, and an officer with several men, on arriving at the house, found the dead bodies of Madame da Costa, her son, and her servant, all three recently assassinated. Julia was still alive, but she had received several wounds in the neck and chest with a dagger, the blade of which was found remaining in the chest; and she lived only long enough to declare that the murderer had been her cousin. Lobo was arrested at his own lodgings. In the lining of a hat were found three cheques on the bank of Oporto, drawn in favour of Madame da Costa, though all the plate and other valuables belonging to that lady had been left untouched. Having been found guilty by a jury, Lobo afterwards confessed in full. After protesting that he had not been urged to the crime by the hope of plunder, he declared that he had committed the murders without any other aid, that the thing was inexplicable to himself, but that he had felt himself driven involuntarily to the act by a momentary impulse, and by a paroxysm of a monomania which had for many years been growing on him.

A man in the French army who had served with distinction in the African war, and been promoted to be a sergeant, but was afterwards degraded to the ranks, was threatened with punishment by his sergeant for some misdemeanour, and the same punishment was afterwards inflicted by

order of a superior officer, though for another cause. At night he was missing at roll-call, and did not enter his barrack till a late hour, when he undressed, and lay down without attracting notice. Suddenly some of his comrades, who were playing at loto, were surprised by his approaching them with a musket in his hand, demanding, "Who is ready to die?" The sergeant entered the barrack-room shortly afterwards, on seeing whom the private levelled his gun, and fired at him, saying that such was in return for what he had said in the morning. The sergeant fell and died; and the assassin approaching him, and ascertaining that life was extinct, ironically apostrophised the body. He, however, opposed no resistance to being arrested, and even took off a decoration he wore, saying that he had no longer a right to wear it; but he declared that "if the time were to come over again he should act in the same way!"

These two cases present a wide contrast. None of the hardihood exhibited by the actor in the latter was manifested by that in the former. The Portuguese had no motives for revenge,—no obvious premeditation of his acts; and he showed no pleasure afterwards at what he had done, to which, indeed, he spoke of himself as having been impelled. The circumstances in the latter case were altogether different. The former may have been a case of homicidal monomania; caprice and sudden determinations, such as manifested by the assassin, are justly enough considered symptoms of imperfect or unsound mind * The latter case was clearly one of murder by premeditation. The law, however, regarded the acts in the same light; both men were executed.

M. Matteucci relates the case of a patient who, having been dosed very largely with sulphate of quinine for an attack of intermittent fever, was attacked by paralysis of the lower extremities. For this, moxas, friction along the spine, and strychnine, were employed. But the practitioner being in a hurry to complete the cure, applied in addition electricity to the spine and lower extremities, which had the effect of bringing on immediate tetanus. A similar event has been perceived on electrifying frogs while under the influence of another of the alkaloids, morphine. We are not told whether the unfortunate patient recovered or escaped further evils.—*L'Experience*, April, 1843.

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* We are told that the cranium and brain of the Portuguese was examined by some surgeons skilled in practical phrenology, and that the convolution in which the organ of destructiveness is situated was found very largely developed.

EXTENSIVE THICKENING OF THE TUNICA VAGINALIS,

WITH EFFUSION OF DARK SANGUINEOUS SECRETION INTO ITS CAVITY;

REMOVAL OF BOTH BY ABSORPTION,

SYNCHRONOUSLY WITH THE

CORRECTION OF DISORDERED FUNCTION.

To the Editor of The Lancet.

Sir,—The following is the case to which I alluded in my former communication (p. 316). I have the honour to be, Sir, your obedient servant,

G. Macilwain.

Argyle-place, June 5, 1843.

A gentleman, about fifty-four years of age, was brought to me January 20, 1843, by an experienced practitioner (Mr. Debatt, of Islington) with a large tumour occupying the left side of the scrotum. He had been troubled with hydrocele, for which he had been repeatedly tapped, and about a pint of the usual straw-coloured fluid had been evacuated. He had also had the hydrocele injected some months previously, but, as would appear, unsuccessfully. The tumour he now showed me was evidently not hydrocele; it was exceedingly firm, very heavy, of rather large size (measuring twelve inches in circumference), and at first conveyed the sensation which would be imparted by a diseased testicle. Examining it, however, very carefully, and with attention to the points of diagnosis which I have recommended,* I gave the opinion that the testicle was probably sound, that the deceptive character of the tumour depended on an altered state of secretion, into the sac of a much thickened tunica vaginalis; and that the only local procedure which I would recommend on the present occasion would be a puncture, both as testing the correctness of the opinion given, and as facilitating any further examination which might be necessary. The patient was fat and corpulent; he had a full jerking pulse; furred tongue; irregular bowels; skin hot and dryish, and occasionally bloody evacuations from the bowels. He has an unhealthy countenance, a sort of pasty-white, shot, as it were, with yellow, occasional headach, and complained much of disposition to sleep. In short, he appeared in such indifferent health that even the puncture appeared to me at the time of questionable propriety; however, as he wished for a more definite prognosis than I could otherwise give, I made a puncture and drew off about three-quarters of a pint of very dark secre-

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* See Practical Directions for the Diagnosis of Inguinal Tumours, &c. &c.

tion, but so much like venous blood that I thought it quite necessary to reserve it for examination. This secretion (for it was certainly not blood) was contained in a very greatly thickened tunica vaginalis; but notwithstanding that the gland felt a little larger, yet I believed that due allowance being made for the dense structure by which it was covered, it was really unaffected. I now told the patient that I thought he would do very well, to make himself comfortable (for he was exceedingly nervous), and that when his health was in an amended condition I would institute some proceeding for the more effectual relief of his local complaint.

He was accordingly ordered small doses of blue pill and colocynth, and these were subsequently replaced by hydr. c. cretâ and antimony; he was desired to be moderate in his diet, and to take a little wine, but to discontinue beer. A catheter was passed a few times, which assisted in relieving some irritation of the urethra; but all these measures, to which were added evaporating lotions, effected no change in the tumour, which was as large as ever. His health had certainly improved, but his countenance was still bilious, and abdomen full. I now recommended that our endeavours should be directed chiefly to the liver, as I was convinced that that organ was principally in fault, but still only with the intention of putting him in condition for the operation. I accordingly prescribed occasional single doses of calomel; but the means ordinarily employed for the purpose intended, were aperients combined with narcotics, almost any of which can be made very effectual excitants of the liver in certain proportions, which will vary in different cases. By these means we obtained large quantities of dark-coloured secretions from the bowels, considerable diminution of corpulency, and evident decrease in the size of the tumour. These measures were continued, and the dimensions of the tumour taken at each visit, until the whole was so nearly gone that I discontinued my visits, begging Mr. Debatt to inform me of the result. At this time very little fluid remained; the tunica vaginalis had nearly recovered its natural condition, and so far as could be distinguished through the yet remaining fluid the testicle appeared to justify the opinion first given. I have since heard from Mr. Debatt, who concludes his letter by saying,—"March 30. Tumour entirely dispersed; there is a little thickening of the tunica vaginalis, but the testicle feels sound, and he experiences no inconvenience whatever, either in the testicle, spermatic cord, or scrotum. His tongue is clean, pulse regular, and bowels healthy; in fact, all his bad symptoms have left him." Although the length of the case leaves little room for remark, I will just observe that it seems interesting in showing what may be done even in regard to thickening of serous membranes; especially as the injection of these cases requires great care; and I have known the attempt to cure them by removing portions of the thickened tunic, in two instances fatal, both of which occurred in London hospitals, in the practice of excellent surgeons.

SMALL-POX ACCOMPANYING VACCINATION.

To the Editor of THE LANCET.

SIR,—A man some years since having a great dread of the small-pox, which, however, was not prevalent at the time, applied to me to be vaccinated. At the expiration of a week, on calling upon my patient, I found that not only had every puncture of the lancet produced a fine specimen of the vaccine pustule, but that each puncture was surrounded by a number of smaller ones, having the same characters, the whole presenting a most beautiful appearance. The man at this time was much indisposed, and directed my attention to the state of his skin, when I discovered the eruption of small-pox just making its appearance. From this time my patient gradually became worse, and, towards the maturation of the pock, purple petechiæ were everywhere interspersed between the pustules. Extreme prostration soon set in, accompanied with much pulmonary implication, together with all the symptoms of malignant disease, and on the seventh day of the eruption the case terminated fatally.

Now, Sir, since variola did not prevail in the neighbourhood at the time, and the case might be considered as sporadic, is it to be inferred that the malignancy of the complaint was owing to some peculiarity of the patient's constitution, which was good, or to the small and cow-pox (two diseases which Jenner believed to be identical) existing at *one period?* If the virulence of the malady be referred to the latter cause, then are we led to this conclusion, that medical men cannot be too cautious in first ascertaining whether those whom they vaccinate have been previously known to be exposed to the actual infection of variola.

Arguing upon the fact that previous vaccination mitigates the violence of small-pox, it might be supposed that the *co-existence* of the two diseases would render the latter milder, but if Jenner's theory of the identity of the two complaints be admitted, I do not see how this can be the case, since it would be a parallel circumstance to a patient taking a second cold while labouring under the effects of the first. I have the honour to be, Sir, your most obedient servant,

ARTHUR TAYLOR, M.R.C.S., &c.

Beaufort-place, Chelsea,
June 3, 1843.

SUBSTANCE OF A LECTURE ON THE STRUCTURE AND FUNCTIONS OF THE ORGAN OF HEARING,

IN RELATION TO SOUND; WITH SOME NEW PHYSIOLOGICAL VIEWS.

Delivered by Mr. CHARLES BROOKE, at the Royal Institution, on Friday Evening, June 2, 1843.

THE lecturer commenced by observing that we can hardly wonder at the extreme complexity of the organ of hearing, when we consider the endless variety of its perceptions;—from the peal of thunder or the roaring cannon to the faintest murmur that floats on the breeze; from the ceaseless din of the manufactory to the undying strains of a Catalani or a Malibran; the ear is organised alike to sustain the intensity of the one, as to appreciate the minuteness, or sympathise with the melody, of the other.

Mr. Brooke remarked, that were he addressing an entirely professional audience he might proceed at once to the elucidation of those points connected with the functions of the ear that he wished especially to bring before the notice of the meeting; but as he felt that he was addressing gentlemen engaged in every variety of literary and scientific pursuit, it would be necessary, in order to render himself intelligible to all, to explain, in the first place, some of the first principles of acoustics, and, secondly, to give an outline of the structure of the ear.

He then stated the perception of sound to result from the impression made upon the auditory nerves by the undulations or vibrations excited in the elastic medium by which we are surrounded, either by the vibrations of an elastic body, or by successive impulses communicated in any manner to the atmosphere; irregular impulses constituting noise, but a series of regular impulses conveying the impression of a definite tone or note: this was illustrated by the sound produced by holding a quill against the teeth of a revolving wheel, and by the *Sirene*, in which a tone is produced by an interrupted current of air passing through holes in a revolving disc, the note becoming more acute as the interruptions occur more frequently. A distinction was then made between the *pitch* and the *quality* of a regular tone, the former depending merely on the frequency of the vibrations, the latter on the nature of the vibrating body; this was illustrated by the sounds of different kinds of organ-pipes sounding the same note.

The different kinds of vibrations were then explained, which it is necessary to be fully acquainted with, in order to comprehend their effects upon the sentient organ of hearing. Vibrations are called transverse when the motion of the particles is *transverse to the length* of the vibrating body, as in the strings of musical instruments; they are called longitudinal, when the motion of the particles is *in the direction of the length* of the vibrating body, as in the vibrating column of air in organ-pipes, and all wind instruments. In relation to a surface, the vibrations are called *normal*, when the motion of the particles is *perpendicular to* the surface; *tangential*, when the motion is *in the direction of* the surface. The relation between *progressive* and *stationary* undulations was then explained, the former taking place when the extent of the vibrating body is not limited, as a distant sound passing onwards through the atmosphere; the latter when the extent is limited, as a column of air in a tube, or a cord fixed at its two ends. The mode in which progressive are converted into stationary undulations, as well as the different kinds of undulations, were very satisfactorily illustrated by experiments and apparatus.

Mr. Brooke concluded this part of his subject by explaining and illustrating what he considered the most important principles of acoustics, in reference to the functions of the ear; first, that however sonorous vibrations may be transmitted, they continue to take place in the same plane in which they were first propagated; and, lastly, the principle of resonance, or the reciprocation of sound; and expressed his regret that time would not permit him to add any more of the experimental illustrations which had been prepared for the occasion.

Mr. Brooke then gave a popular outline of the anatomical structure of the ear, expressing his apprehension that from its extreme complexity he should scarcely be able, in so short a space of time, to render that structure intelligible to those who were previously unacquainted with it; this it is unnecessary to repeat here; we shall, therefore, pass on to the last part of Mr. Brooke's communication, comprising his views of the functions of the different parts of the internal ear.

1. The mode of transmission of sonorous vibrations to the labyrinth.—The vibrations passing through the atmosphere are communicated to the tympanum and thence to the air in the cavity of the tympanum, and in the mastoid cells, which, it may be here remarked, constitute a very irregular and broken cavity, in order that *any* sound may be readily reciprocated; were the mastoid cells more approaching to a single regular cavity, they would resound more readily to one particular tone, or to a very limited extent of tones, to the comparative exclusion of all others. This is frequently observed to be the case as we descend in the scale of animal organisation, where the natural tones of the animal are very limited, and where it is essential that they should be more readily

perceived than any other sounds. The vibrations in the cavity of the tympanum are communicated to the membrane closing the fenestra rotunda (just as the vibrations of the external air are communicated to the tympanum itself), and thence through the fluid filling the labyrinth to the semicircular canals and cochlea, which, in consequence of the foramen existing at the apex of the cochlea, constitute one *undivided* cavity. The transmission of sound, it was considered, cannot take place through the *ossicula*, as was formerly supposed, inasmuch as the plane in which the crura of the stapes lie is exactly perpendicular to the plane passing through the manubrium of the malleus and the long leg of the incus, and consequently the vibrations travelling through these would be almost entirely intercepted by the stapes. And, moreover, it appears essential that the transmission of vibrations through the chain of ossicula should be prevented; for as the velocity with which sound is transmitted through solids, such as bone, so very far exceeds its velocity in air, if the same undulations reached the labyrinth through this channel, and through the channel previously described, they would reach it in different times, and a constant interference would be produced.

2. The functions of the *ossicula auditûs*.—This osseous arrangement Mr. Brooke considered to perform an office in the ear analogous to that of the iris in regard to light, namely, that of regulating the tension of the various structures that are thrown into a state of vibration, according to the pitch and intensity of the sound to be transmitted to the sentient nervous fibres. This was conceived to be effected by the conjoined action of the *tensor tympani* and *stapedius* muscles, by which the tympanum would be rendered more tense, and a simultaneous change in the position of the *stapes* would alter the tension of the fluid throughout the labyrinth, and, therefore, also the tension of the membrane of the fenestra rotunda, which intervenes between that fluid and the air in the tympanic cavity. It may, perhaps, be still questionable whether the alteration in the tension of the fluid may be sufficient to affect the transmission of the sonorous waves, but, at all events, that mechanical power may be considered as sufficient to effect the requisite alteration in the tension of the membrane of the *fenestra rotunda*. This train of involuntary muscular actions is supposed to be set up by the impulse communicated to the *chorda tympani* through the *processus gracilis*, which lies in close apposition to it, when the first dash of the sonorous wave reaches the tympanum; just as the iris contracts through the agency of the ciliary nerves, in obedience to the stimulus of strong light falling on the retina; and it may be further stated, in confirmation of this view of the subject, that all those functions performed by the various organs of sense that are essential either to the natural exercise of the organs themselves, or to the preservation of their integrity, as well as the vital actions of all the organs of the body, are performed through the agency of the ganglionic system, to which the chorda tympani belongs.

3. The function of the semicircular canals.—These are universally found to lie in three planes, perpendicular to each other, and as the sonorous vibrations have been shown to be transmitted in the same plane in which they are first propagated, the relative intensity of the vibrations in the different canals would depend on the direction in which the wave proceeded, and hence the most important function of these canals has no doubt been correctly inferred to be the discrimination of the direction from which sound proceeds. It was remarked that this structure is met with as low in the scale of organisation as the fishes, and as fully developed as in man, and this fact is quite in accordance with the function that has been assigned, as it must be of the greatest importance to the preservation of animal life to be aware of the direction in which danger is threatened.

Lastly, the function which Mr. Brooke proposed to assign to the cochlea was that of discriminating the pitch of sound, or the particular tone produced. This he conceived to be effected through the agency of the transverse nervous fibres constituting the *lamina spinalis*; these are observed to be of gradually diminishing length from the commencement to the apex of the lamina, and the vibration of each individual fibre, or rather loop of nervous matter, he supposed to bear some fixed relation to a particular note. This view he considered to be supported by the fact first noticed by Dr. Wollaston, that there is a limit in the acuteness of audible sounds, and that that limit varies in different individuals. This might be inferred from a variation in the ultimate length of the nervous fibrillæ, or, in other words, of the terminal diameter of the spiral tube; a variation which is as likely to occur as that of any other feature in the frame.

The lecture, which was illustrated by a great variety of experiments, afforded, together with these, much interest to a numerous audience.

Dr. ADELMANN, a practitioner in Bavaria, is employing, very successfully—as he says—in inflammation of the tonsils and pharynx, various preparations of *arnica*, both in the form of gargles and by the stomach. He also recommends the inhalation of vapour from a hot infusion of mustard-seed.

THE LANCET.

London, Saturday, June 10, 1843.

THE complaint that hospital physicians and surgeons furnish the fewest possible contributions towards the improvement of medical science has been frequently made in this Journal. An instructive and original work, which is reviewed at page 386, of this week's LANCET, "On Spasm, Languor, Palsy," &c., by JAMES ARTHUR WILSON, M.D., physician to St. George's Hospital, affords us the opportunity of making an exception to this grave charge of betraying one of the highest trusts in a civilised community, and we admit the exception with alacrity; for, while we execrate the imbecility, the inertness, the waste of opportunity, and the recklessness of the interests of their fellow-beings, which characterise so many officers of our public medical charities, we are ever ready to honour the individual who avails himself of the invaluable means that are confided to him in those establishments, to present the proceeds of his experience,—not saving them for his own gain alone,—to the less fortunate of his brethren and the world at large, in order that they may also profit by his reflections and labours.

Dr. WILSON's volume, although but a duodecimo of two hundred pages, comprises one of the boldest attacks upon the more prominent doctrines of the British medical school (we are aware that there is a large and increasing proportion of the profession who will read "prevailing errors") which has yet been made. However quietly his assault appears to be made, yet it is not on the outposts alone, nor with a light skirmishing manner. The main forts of the edifice are boldly assailed. The doctrine of the *supremacy of the nervous system* in the animal structure is forcibly attacked. It is true, that the materials of the fabrics that he knocks to pieces are to be incorporated in a new edifice; that in his explanations of disease Dr. WILSON allows that the nerve, being an integral and necessary part of structure, must be duly considered in debating treatment, and that diseases of the nervous centres may occasion, and, in a certain degree, may be measured, by functional disturbance in the muscular structures; and that the muscular function may at times be taken as the exponent of diseases affecting the organisation of the brain and spinal marrow, as illustrated in spasm, palsy, convulsions, and the like; but this physician abjures entirely the *necessary dependence* of these phenomena upon the nerves, or the assumed nervous power, referring them to the inherent vitality, or "irritability," as it is called, of the fibre.

"The nervous structures are appended, and secondary in their nature, to the self-contracting fibre." "There is no common function of the nerves." The expression "nervous system" is true only in its anatomical sense. "No common influences are transmitted through the nerve to general structure, excepting such as have been received by the central nervous structures from the blood; the nerves vary in their functions, as in their anatomical character, with the great organised divisions of structure from which they originate." P. 58.

Dr. WILSON thus repudiates the explanations at present admitted by physicians, of one half of the diseases and of one half of the symptoms of the remaining half, constituting a modification of the theory, at present in vogue, of nearly all the maladies which flesh inherits.

The work is the very antithesis of systematic medicine, according to the present usage and acceptation of that term; yet the empiric will find the volume worse than useless to him, and it is to the rational practitioner only that we can recommend its perusal. One great aim of the author is to urge upon practitioners the importance of attending to muscular action,—to "the muscular function in its completeness and integrity."

"It should be carefully remembered that there may be great disturbance of the muscular function, for which neither the brain nor the nerves of the affected muscles are in any way responsible."—P. 3. "Neither in health nor in disease is the muscle to be regarded merely as the organ of motion, as ministering only to an occasional and mechanical function; but collectively, and in mass, as the most extensive of living struc-

tures, as continually employing and employed upon a large proportion of the entire mass of the blood, thus elaborating the great common material of the body, and preparing it for circulation elsewhere. This is true of the muscle at all times, whether in action or repose. It is a necessary result of the elementary business by which the muscle lives and grows—of the common healthy nutrition of its own peculiar structure."

The independent power of action possessed by muscular fibre, shown by the faculty, in living structure, of reproduction from the same common material—the blood—is pointed to as of the highest importance in pathology. By muscular action, however, in the observation before us, is never meant less than a result of the triple agency of nerve and fibre with blood. The muscular fibre is influenced *directly* by agencies, both from within and from without—by a channel much more direct than nerve—"the continuous universal blood." This is illustrated in innumerable instances. Spasms may be induced by substances introduced into the blood; paralysis, or the suspension of muscular action, by the absence of principles in the blood, the effects being directly upon the muscle, not through the intervention of nerve. If states of the blood imply states of the muscle, in the full development of diseased action, the converse of this rule is equally true:—

"Medicine and disease work, it is ever found, on the same living material by the same means of living action, and thus it follows that sometimes by extending the supply of blood to the muscle, sometimes by reducing it, by removing hurtful principles from the circulation, or by restoring those that are inherently wholesome, we do, in truth, best control the symptoms of muscular disorder."

Spasm, in its worst forms, seems to have least to do with the nerves of the affected muscles, and is compatible with the soundest state of their structure. It is from what offends the blood in the fibre that spasm in the voluntary muscles is for the most part induced. On the other hand, there is no state of the "nervous system," in its structure or function, with which spasm of the voluntary muscles, however violent, is not compatible. Many spasmodic affections depend upon *impaired nutrition* in the structure of the affected muscles, and interruption to the current of the large blood-vessels, rather than does compression of the trunk of the nerve account for cramp from pressure.

Some interesting and instructive remarks will be found under the head of "Langour." This state is defined to be "a distressing "sense of inability to originate motion, dis-"tinct from actual weakness," page 144. It is well described by the author from nature. Langour operates on the nerves, through the blood, or from the structures of which they, the nerves, form a part. It should be considered with reference to the volume and quantity of the blood, and the forces which sustain and regulate its current. In its treatment the worst means are those in general use.

Palsy, again, for the most part depends upon faulty relations between the blood and the brain, the latter being rendered powerless in the muscle. "He who drinks him-"self to death begins by poisoning his own "blood." "Palsy of the muscle does not "always originate from central injury or "congestion of its nerve. Instances are "not unfrequent, in these days of pauperism "and starvation, of persons dropping sud-"denly by the way-side, helpless and in-"articulate, from sheer inanition of the "muscular fibre."

The undoubted fact that when the blood is most charged with narcotic principles, as in poisoning with hydrocyanic acid, the cold affusion is one of the most powerful and advantageous remedies, leads to the inference that even that agent produces its effects through the blood, in mass and in current. In truth, Dr. WILSON'S invasions upon the hitherto exclusive ground of nervous pathology are universal. Death from an extensive injury of the skin does not result from irritation of the cuticular nerves, but from the circulation of damaged blood through its fibre; and even the mind first expresses itself in the body by the muscle and through the blood.

Dr. WILSON avows himself, with caution, to be one of the chemical school. "By "chemistry it is now proclaimed that "we are for the future to learn the "body through the blood," and "from the "animal chemist it is probable that our "greatest gains will henceforth accrue." Importance is attached to the chemical correspondence between muscular fibre and the coagulating principle of the blood. Spasm, it is suggested, by delaying the blood's current, may be a means of relief. This strong action of the muscles may be for good, influencing directly the mechanism of

the circulation, and inducing further chemical changes by waste of oxygen in the blood.

It may thus be well understood that, according to Dr. WILSON, the nerves of the animal body have been mistaken as to their functions, or the part that they play in the great business of life; and that the nutrition of the muscular structure is the source of that power, in the application of which the nerves perform a secondary part. It is a remarkable circumstance that LIEBIG, regarding the body from a more strictly chemical point of sight, considers the source of *vital power* to lie in the *nutrition* of vital parts, and particularly in that of the muscular tissue. The coincidence is the more remarkable as we believe Dr. WILSON taught these doctrines twelve or fourteen years ago. The Doctor follows out the subject physiologically and pathologically, and the Professor chemically and physically. Both taking the same point of departure, the one records the results of his own observation at the bed-side of the patient of the symptoms of disease; the other compares the force whose origin he has traced with other forces, and refers it to the doctrines of force which are universally admitted to be eternal truths, applying the principles of natural philosophy to the vital force. We conceive that very little doubt can exist in the mind of the philosophic reader, that in the progress of science these two paths of investigation must approach each other, and ultimately conjoin, to form one broad road to the temple of truth, although at present, as LIEBIG has remarked, we may not see the connection.

Our object in this second notice of Dr. WILSON's work is to promulgate his views—not to pronounce upon them; but we are inclined to believe that Dr. WILSON will not willingly be understood by every reader. This, however, we may say, that those who have studied the "Chemistry of Animal Life" of LIEBIG will most easily comprehend the bed-side philosophy of Dr. WILSON, and we may considerately say that as LIEBIG has proved himself to be in advance of the age in the chemistry and physics of the animal body, so Dr. WILSON will be found to have anticipated his contemporaries in some important improvements in physiology and pathology.

THE reform of prevalent abuses in the BRITISH MUSEUM, a matter which we briefly noticed last week, is probably not so difficult and distant as changes for the better in literary and scientific institutions were twenty years ago. Precedents and examples in reform are now citable in many of these establishments; and managers, and trustees of all kinds, both civil and political, love *precedents*, though they always wait for them instead of being early in the field to create them. Those of the British Museum will find some in our schools of medicine, where the lecturers and pupils, who had formerly so few interests in common (the former contenting themselves with the smallest possible exertion in teaching, the latter never encouraged to step beyond the least modicum of attainment which would "carry them through" the Hall or the College), are now uniting their energies to raise medicine to at least a respectable rank among the *learned* professions. Museums have been formed at the hospitals on far more extended scales for the illustration of structure, healthy and diseased; and stimulants are offered, in the shape of prizes and scholarships, to industrious students, though adjudged in a very absurd and unsatisfactory manner. Even the College of Surgeons has marched a few steps in the road of reform. The Hunterian Museum has been much enlarged, and illustrated by creditable scientific catalogues, and the library is now thrown open to the members at large,—scarcely creditable, though, that it was ever closed against them,—and is supplied with new works, British and continental. (Why is it not opened to students as well as members? The latter have the most time for reading.) The Apothecaries' Company has done something in its little way to keep level with the times, so far as a bad Act of Parliament was interpreted to allow the worshipful court of assistants. In the provinces, also, quite a new face has been put on medical establishments. In short, every where the class of jog-trots is giving way. In the British Museum, however, it is desir-

able that they should yield a little faster, and as the subject falls legitimately under our notice, we willingly render the reformers of that institution a little help to effect their purpose.

The British Museum is a national institution, supported by the country at an annual cost ranging between 16,000*l.* and 21,000*l.* (exclusively of grants for building, &c.). The whole community, therefore, ought to enjoy the fullest possible use of the enormous amount of property which is available within its walls for the promotion of literature and science,—its vast collections from the animal, vegetable, and mineral kingdoms, its splendid library of three hundred and sixty thousand printed books, and its invaluable manuscripts. The very fact, indeed, that the trustees possess the important privilege (conferred in the time of GEORGE II.) of claiming a copy, gratis, of every work that is published within the United Kingdoms, should render it essentially a *national* library. And, with the view of setting this subject more fully before our readers, let us enter into some details respecting the present state and effectiveness of the British Museum.

First, with respect to its Library and Reading-room. The Parliamentary inquiries of 1835-6, which revealed so many awkward secrets of the fraternity of managers in this establishment, induced the *diffident* trustees to extend the hours for reading, during summer, until seven in the evening, thus allowing ten hours, daily, for study during that season. But why not extend the hours from nine to nine all the year round, and use gaslight in the reading-room, providing that the orders for the books that are read by the frequenters be always executed in the daytime, an arrangement which would at once obviate all danger from fire, the more especially as the rooms are all separately fire-proof? Were this plan once adopted (and considering that the daily readers now average two hundred and fifty, it *must* be adopted, sooner or later,) the doors, now effectually barred to large numbers of readers, would be open to men who are engaged in the learned professions,—men to whom, above all others, such a library would be valuable for study and research.

But the catalogues require great reform. The catalogue of printed books alone (exclusive of those of GEORGE the Third's library) occupies forty-eight folio volumes; and when the enlarged edition is completed they will be swollen into one hundred and twenty; and yet with all this unwieldy mass the arrangement at present is merely *alphabetical*, with no classification of *subjects*. Some years ago several hundred pounds were expended, under the direction of the Rev. H. H. HORNE, and other bibliographers, in preparing a classed catalogue; but after the work had proceeded some way the scheme was abandoned, for reasons known well enough to the trustees and officers, but little satisfactory either to the Committee of Inquiry or the hosts of students in the rooms, who now spend entire hours, nay, even days, in looking for works with which they may be well acquainted, though they are not able to produce their exact titles, as given in the catalogue. This is an old and very serious grievance, which, now that a new alphabetical catalogue is printing, we almost despair of seeing reformed; but there is another, which might easily be removed by the energies of so active a librarian as Mr. PANIZZI, aided as he is by a positive army of scribes and attendants. We allude to the delay that takes place between the receipt of a publication at the Museum and its appearance in the room-catalogue, several months frequently elapsing before a work that is actually in the house can be obtained by the readers. A *special inquiry* on the part of some more urgent applicant, it is true, may end in the production of the book by the collector; but few persons are forward enough to press their demand and after all, the obliging conduct of an individual attendant is but an insufficient atonement for the neglect of the librarians at large! To the same cause, namely, neglect on the part of the upper officers, may be ascribed the tardy collection of new books, the inconveniences of which are almost daily felt by the great body of readers, or, at any rate, by all who require to be *au niveau* of the literature of the day.* But, independently of new books, very many good old medical

* The Museum has an unequalled collection of newspapers, but, owing to some absurd regulations of the Stamp-office, newspapers do not make their appearance here until at least *two* years after publication, and sometimes longer.

works of high repute, which are in the libraries of the College of Surgeons and the Medico-Chirurgical Society, are still *libri desiderati*, as respects the great national library of England; and hundreds of valuable continental works and periodicals on every subject, but on medicine in particular, are not to be obtained in any shape at the British Museum. Surely it would be better to supply these glaring deficiencies, than lavish 2,500*l.* yearly on bindings better suited for a lady's boudoir or the shelves of a fashionable breakfast-room, than for the hands of hard-working students.

Secondly, with respect to the collections connected with natural science, magnificent as they are, the catalogues, throughout, are so framed as to prevent them from being of the smallest use either to the public at large or to students, being too scientific for the former and too meagre for the latter. An attempt, indeed, has just been made by the officers (in compliance with Resolution 12 of the House of Commons' Report) to meet the views of the million by cutting up the shilling synopsis into minor sections; but this is a miserable half measure that will satisfy no party, nor can the "synopsis," even in its present form, be termed "so complete as regards what is exhibited that there is little necessity for visitors to ask questions." (See Sir H. Ellis, before the House of Commons, on National Monuments). The only plan, indeed, is to furnish, at a cheap rate (say at sixpence), a guide so familiar that he who runs may read, for the use of general visitors,* and to produce a separate and really scientific catalogue that would furnish the student with full information on the structure, habits, and uses of the objects presented for his examination. To make such catalogues is the duty of the officers attached to the various departments; and if, through want of health, talent, or disposition, those officers now holding office shrink from the task, others should be appointed to it who will bring the work to a speedy completion,—"men of eminence in literature, "science, and art, by whom such appoint-"ments would be regarded as marks of dis-"tinction." (Commons' Resolution 5.)

But the want of catalogues is not a solitary deficiency. In such a museum, attention should be paid to the demonstrations of comparative anatomy. A comparison of the bony structure of animals might be made generally interesting by a proper arrangement of skeletons, and it would not be difficult to show the distinctions of animals by teeth, feet, digestive organs, &c., towards which not the shadow of an attempt has been made. It has been objected, that such distinctive demonstrations are properly confined to museums of medical science. The remark is worthy only of the grandmothers of museums. The knowledge would not only be highly interesting, but would lead to habits of classification and order, which are most important to all persons. Moreover, a few only of the specimens of lower animals are exposed to public view, the rest being confined in drawers, and open to examination merely by those who may chance to know that they exist there, and have interest with the officers to procure the privilege of a private inspection. The Botanical Museum (besides a very extensive and valuable collection, comprising the Sloanian, Banksian, and other herbaria) is wholly closed against the public; and the arrangements of it are such that, in point of fact, its doors are closed also against the great mass of medical students, to whom a knowledge of botany is an absolute prerequisite for the licence to practise. If this portion of the Museum were open, like the reading-room, hundreds of students would avail themselves of its treasures; and, under proper regulation, the plants, or a certain practically useful portion thereof, would no more be injured by use than are the books in the library.

With the coins and prints we have less business; but amongst those objects, also, the same exclusive system prevails. Indeed, exclusiveness is the pervading spirit of the whole institution and its managers, from the Archbishop of Canterbury (the principal trustee) and his too faithful satellites, Sir Henry Ellis and Mr. Forshall, down to the humblest attendant in the house. Every step towards reform has been forced from them by a species of moral torture, and it is only just

* Since making these remarks we have found on sale at the door of the Museum, in Great Russell-street, a little volume entitled "The People's Hand-Book to the British Museum," published by D. M. Aird, at 2, Tavistock-street, Covent-garden, at sixpence, which is calculated completely to supply this deficiency. It has evidently been written by persons who have made themselves masters of the subjects displayed, and known what to do with the materials under review.

now that the public is awakening to the fact that the institution is not the property of the officers, but simply committed to their care for the use of the nation at large. Indeed, till within the last half-dozen years, the great mass of the community has been perfectly childish as regards its rights in national institutions and the duties of the parties who fill the offices of public servants therein; but, assuredly, the time has at length arrived when the officers of the British Museum, as well as all other public establishments, should be made to feel that they are to *subserve*, and not *impede*, the national interests.

On Spasm, Langour, Palsy, and other Disorders, termed Nervous, of the Muscular System. By JAMES ARTHUR WILSON, M.D., Fellow of the Royal College of Physicians, and Physician to St. George's Hospital. London: J. Parker, 1843. 12mo, pp.

FONTENELLE said "that all philosophy consisted in possessing a great deal of curiosity and very bad eyes." There was much truth in his observation. The demonstrative sciences afford ample proof that the assertion was correct, and if we bear in mind the various names that have been given to different parts of organs, in compliment to those who had developed their structure, we shall see that it has required centuries to become thoroughly acquainted with that which has been wide open to observation from the beginning of time.

The structure of the eye has ever been what it is at present. Its parts are not very minute, nor very numerous; yet it has required the successive labours of many anatomists to develop its organisation, with the discoveries in which are honourably associated the names of Petit, Willis, Sœmmering, and others. Where function has implied suitable organisation, the point has been conceded, *ad interim*, until better eyes have reduced hypothesis to tangible form and figure. And what do these modern discoveries prove? Not how much has been done by patient and philosophical inquiry, but how much is *still* to be expected by a continuance of those endeavours, which, trusting, like faith, in what is unseen, are finally crowned by bringing darkness to light.

The little volume of Dr. Wilson contains a great deal that is philosophical and much that is interesting; but we cannot admit of that exclusiveness of action in the production of phenomena which the author is inclined to attribute to the blood alone; and when he states that "We often hear of spasm in structure where no muscle has been demonstrated by the anatomist," we may state that the anatomist himself has yet much to do; and if we allow of muscularity in some structures, on the authority of their functions, we may ascribe spasm to their existence in others, excited, as it may be, by the influence of blood or nerve.

Dr. Wilson has found, in his own experience, that those fevers which are ushered in by spasm become eruptive in their progress, and instances cases of spotted epidemic fever, varioloid fever, and chicken-pox, in corroboration of his assertion. This manifests, by anticipation, the strong tinge of humoral pathology with which these pages are imbued. The author lays no claim to novelty in the statement just quoted, as it is a well-recognised fact; for Cullen observes that small-pox is frequently ushered in by an epileptic fit, and Dr. Gregory laid very great stress upon this curious circumstance in his lectures on the exanthemata.

We require to be reminded of such important pathological truths; for the rarity of the small-pox since the introduction of the vaccine has allowed, perhaps, but few persons to become practically acquainted with them; and the experience of others glides away from our memories, unless the impress is renewed by occasional repetition.

The influence of mercurial vapour in producing convulsions and paralysis admits of more general observation, and is no where better exemplified than in the sad condition of the workmen who are employed, for instance, in the gilding of the cupolas of churches, of which we have seen many and lamentable instances. The gold is laid on with mercury, and the latter volatilised by heat, and many a sturdy fellow is doomed, in the prime of life, to drag on a miserable existence for the remainder of his days, shaking with palsy, unable to convey his hand to his mouth, after the inhalation of this poisonous vapour.

These are some of the instances which are adduced of spasmodic action and convulsion caused by the influence of blood deteriorated by the diffusion of poisons through its mass.

"Spasm, in all cases, excepting those of direct local injury, implies a further effect of

disorder in the system than is expressed by the irregular contractions of the affected muscle." * * * * "In shaking palsy induced by mercurial poison the spasm is of this pervading kind. It is but one symptom among many of the constitutional disease, and is associated with evidence of disordered action in every part of the system."

Cramp, which affords the best illustration of spasm in the double voluntary muscles, is not considered by Dr. Wilson as ever deserving the name of an idiopathic affection,—that is to say, occurring without constitutional disturbance. After stating that spasm is not necessarily connected with disease in nervous structures, nor with abscesses in the brain, tumours, or ossification of the membrane, the author assumes that "it is from what offends the blood in the fibre that spasm of the voluntary muscles is for the most part induced? There are no cramps," he adds, "so severe as those of which poison mixed with the blood is the direct cause." This affords the author an opportunity of reviving the Hunterian doctrine of the life of the blood, which he does in very energetic language:—

"Can we refuse the inference that always in the muscles, when thus disturbed, there is a prejudice of the blood by composition or in function, and by the blood's function how much is implied—how much more than the little we know of it by its material composition or in its mechanical relations? This has not been enough considered. The blood not only lives, but its life predominates over that of all else in the system. It lives in the mass as in the particles, as an organ, that is, with a combined function; as an organ, therefore, universal in the body; for the blood in the living body is everywhere, and everywhere at once. As the air surrounds us from without, so does the blood pervade us, like an atmosphere, from within, receiving and transmitting influences through its continuous mass, which are acknowledged in all parts of the system. Most of what is effected by the blood is thus accomplished,—not by the direct admixture of material principles with its current, and the subsequent transport of such principles to the several organs,—not by chemical agencies thus conveyed,—but by action inherent in the blood itself, rapid and universal as electricity, occasionally developed by the mere contact of certain forms of matter, or by agencies that are immaterial."

Again,—"It is not so much from what is added of *material* to its composition, as by what it receives of impulse to its action, that effects follow from the blood to the system. Cramp is among those effects, which are most frequent in those structures where, as in the muscle, the blood is most abundant. Self-moving, self-producing, maintaining its own fluidity, arresting its own current by self-coagulation, the blood, in its wide range of capacity, is affected directly and at once by the countless agents of vital impression. Hence, from these varying states of blood, in the flesh, proceed the various symptoms of disturbed action that are usually termed 'nervous,' in the voluntary muscles."

Although we assent to the principle, as a general proposition, that the blood possesses properties which place it in a very different light from fluids passing mechanically through tubes, but which are endowed with no special qualities, still we are not prepared to attribute so much to the blood alone, nor to endow it with such independent properties. We must remember that that fluid is propelled through living organised tubes, which retain their vitality after the blood that escapes from them loses it, and that the blood becomes *defunct* as soon as it is effused,—is decomposed, is an *extraneous body*, is *a heap of refuse*, and requires the operation of *vital scavengers* to remove it. It is positively certain that noxious matters diffused through the blood do excite spasmodic action in muscles, and we cannot admit the occurrence of spasm where no contractile fibre exists. Still, we have seen many cases of idiopathic spasm where there can have been no question respecting the existence of deteriorated circulating fluid. In their younger days many persons never dance without suffering subsequently from cramp in the calves, and still have the misfortune to be severely pinched in drawing off long boots, being compelled to halt with one leg in the boot-jack. Indeed, some individuals can produce cramp, at any time, in the great toe, by bending it down upon the sole of the foot. In all such cases we would ascribe a due share of influence to the blood, but that only of a mechanical kind. To the nerve which throws the muscle into contraction we must look for the *primum mobile*, and we think that Dr. Wilson has philosophically suggested the cause of the pain when he says that "it may possibly depend on the contraction in opposite directions of unequal segments of the affected muscle." We certainly believe in the existence of idiopathic cramp where the blood is in its natural state, but consider that it is excited by a deteriorated condition of that fluid, and that, as the author justly observes, "There are no cramps

so severe as those of which poison mixed with the blood is the direct cause." No spasms were more terrific than those that were witnessed in some cases of the cholera, a disease arising, as we fully believe, from a specific poison diffused through the blood, producing its malignant effects on the nerves and muscles. We once witnessed the cure of a paralysed limb by the violent universal spasms which occurred in a case of cholera. An old lady had almost lost the use of her left arm for years, and reported that she had suffered a slight paralytic stroke. She was attacked with spasmodic cholera in its most painful form, the cramps in the arms and legs being most severe, but she finally recovered, and completely regained the use of the paralysed arm.

Muscle, nerve, and blood, form an indissoluble trinity, and healthy action depends upon the due proportion of labour allotted to each. It is impossible for one to be deranged without affecting the others. Action and reaction may occur between them, producing derangement, or, as we have seen, restoring function. The paralytic limb loses part of its animal heat, its muscles become flabby, and its circulation less active, and the blood which flows through it may feel this impress, but the palsy may have been induced by mechanical means, the removal of which is followed by a restoration to its normal condition. The nerve shall regain its irritability, the muscle its tone, and the blood its momentum. In other cases the blood, poisoned by extraneous matter, as by strychnine, shall become the prime offender, and, acting upon the nerve and muscle, excite them to extraordinary action. Still they are but meshes of the same web. Tread upon a fibre and the whole vibrates.

Dr. Wilson wishes to rescue *tetanus* from the class of the *neuroses*:—

"By thus considering tetanus in its wide and constant relations with the blood, we cannot fail to know it in its true character of a great constitutional disorder. Because it begins and ends with spasm of the voluntary muscles, tetanus has hitherto been classed with affections of the 'nervous system,' an undue limitation of the disease, for which there is no sufficiant warrant by the symptoms, or in its general pathology. Like fever, it pervades the entire system, and is special in the flesh, being common in the blood. A fever in truth it is, spasmodic and remittent in its character. As a fever, it proceeds from local injury or constitutional disturbance, requiring for its full development a certain period of previous incubation. * * * * In familiar phrase tetanus may be termed the *muscular fever* of pathology."

We doubt whether this will meet with general assent as regards tetanus in its traumatic form, which the author does not exclude in his general view of the disease. It is here that we obtain the fullest view of the author's opinions. Especially in this part of the volume is it that the challenge is given,—Blood *versus* Nerve:—

"By no irritation, by no torture, however ingeniously devised, of any nerve, or of any number of nerves, in the body, would it be possible to establish a complete tetanus in the muscular structure under a given period of time, varying from forty-eight hours to three weeks."

What all the tortures of the rack and screw cannot accomplish under a certain number of hours (forty-eight being the minimum), is produced in as many seconds through the medium of the blood:—

"We have seen, on the other hand, that in a few seconds a tetanic spasm, universal and fatal, of the entire range of the voluntary muscles, may be induced by the introduction of a few drops of poison to the blood."

This is sanctioned by all the experiments which we have seen where poison has been introduced into the veins of animals. Still, it is upon the nerves that the poisoned blood produces that influence which causes cramp and spasm, these also being produced, if not so readily, still more quickly than the author admits, by simple lesion of a nerve. Dr. Gregory used to relate the case of a waiter whose thumb was lacerated by a piece of an earthern dish coming out as he lifted it from the table; spasms, convulsions, locked-jaw, ensued, and he died on the same evening.

We must not indulge longer in discussions which seldom lead to conviction, and only lament, with Fontenelle, that we have not better eyes to help out our philosophy, seeing through all our magnifying glasses but very darkly as yet. Let us observe more closely, studying the facts which present themselves—and this volume abounds in facts of very great interest.

The author directs attention to renal disease and its consequences, and affords proofs of the neglect of due investigation and want of proper discrimination in treating

those cases. It is very satisfactory to quote such passages as the following:—

"Whoever has seen a patient comatose from retention of urine is aware of this dependance of the brain for the proper discharge of its function upon the kidney; yet few care to understand how rapidly, entirely, and fatally, the gland may influence the nerves in their central assemblage, which is the brain."

It is surprising that the experiments of Darwin, when prosecuting his inquiries under the impression of discovering a direct communication between the stomach and the bladder, should not have directed the attention of pathologists to this important point. The ureters of dogs were tied with ligatures; the animals soon became convulsed and died, and upon opening the head serum was found effused upon the brain.

The volume contains many things that are deserving of perusal and investigation; but we must conclude, at present, with this important quotation, our limits not allowing us to treat upon many practical points, such as the treatment of cholera, to which Dr. Wilson directs attention:—

"My inference from these cases of sudden death from renal disease, coincident with integrity of the cerebral structures, is, that in the treatment of epilepsy, anasarca, and hydrothorax, of apoplexy, and disorders of the pulse and breath, the structure and function of the kidneys should be taken well into the account. Moreover, that in all dissections of persons who have died suddenly, the kidney should be carefully examined; and this, whether the heart, brain, lungs, and large blood-vessels be sound or not; for disease of the kidney is often the cause of organic disease in the vital structures, influencing their nutrition through the common material of the blood, upon the elaboration of which the kidney is unceasingly employed."

SOCIETY FOR RELIEF OF WIDOWS AND ORPHANS OF MEDICAL MEN IN LONDON AND ITS VICINITY.

The annual dinner of this society took place last Saturday at the Freemasons' Tavern, and was attended by about seventy of the members and their friends. The Duke of Cambridge presided, and after dinner called attention to the fund for the relief of the families of deceased members who might have failed in struggling against the difficulties of their profession, or been worn out in its practice, and died without realising a maintenance for their surviving relations. His Royal Highness said that he felt assured that such an institution was exceedingly necessary, from the very great difficulties with which medical men had to struggle, and the casualties to which they were exposed, and he was most happy to find how much this society had done to relieve the claimants on its funds. He then read part of the annual report, from which it appeared that the society had relieved within the past year thirty-one widows and twenty children with nearly £1300, in half-yearly payments, and that £385 had been added to the capital stock, which now amounted to nearly £41,000. His royal highness hoped that many would prove, by their contributions that evening, that they felt as he himself did, warmly interested in the prosperity of the society. On his own health being proposed, he added that he was gratified to express thus publicly his best wishes towards the institution, and as it had been intimated to him that he could serve its interests by becoming the patron he accepted that office with great pleasure, and should at all times willingly promote the society's welfare.

Mr. Bacot, the acting treasurer, reported that the investment of the capital with the Commissioners for the Reduction of the National Debt, had already produced a considerable increase in the half-yearly income, and that the present condition of the society was certainly flourishing; but that it must be borne in mind that, according to calculations made long ago, and verified by all subsequent experience, the number of claimants, although actually smaller this year than in the last, would continue to increase for the next twenty years. To meet this demand, therefore, and to secure stability and permanent efficiency, would require the most careful management on the part of the directors, and the utmost exertions of friends and supporters. The recent investment with the commissioners was suggested by three members, viz., Dr. Mann Burrows, Mr. Dover, and Mr. Maclure; of the exact order of priority he could not, however, be sure.

Sir H. Halford briefly responded to the toast of the Royal College of Physicians, and professed his cordial attachment to the society, and called attention to the generosity of the Princess Sophia and the Duchess of Gloucester, who had each again sent ten guineas, that of the Princess Sophia being her twentieth of the same amount.

Mr. Bacot returned thanks for "the College of Surgeons," regretting extremely that no member of the council was present.

Mr. Drew thanked the company for the toast of "the Society of Apothecaries," and announced the repetition this year of their annual donation of fifteen guineas.

Sir Charles M. Clarke, for the vice-pre-

sidents, returned thanks to the chairman for his presence on that evening, and remarked on the readiness of his royal highness to come forward in the cause of charity, leaving the luxuries of his elevated position to preside for the advantage of philanthropic institutions.

On the Duke leaving the chair, it was taken by Sir H. HALFORD (the president), and subsequently by Sir C. CLARKE, V.P. The memory of the Duke of Sussex was drunk in "solemn silence." Dr. MANN BURROWS suggested a closer attendance of the directors at the various courts of the society, and pointed out that their benevolence had sometimes exceeded the limits of prudence. Dr. WILSON good humouredly denied that they had neglected their duty, and expressed his hearty wishes for the success of the society, hoping that some day, instead of meeting at a coffee-house in Holborn, its councils would be held in the hall of a Royal College, and the society embrace every member of the profession in the British possessions.

Amongst those present were Sir James Eyre, Drs. Cape, Forbes, Harrison, Jeaffreson, Mayo, Merriman, William Merriman, Nairne, and Warburton, and Messrs. Alexander, Blagden, Dumergue, Fisher, Hammerton, Cæsar Hawkins, Hawkins, Johnson, Lane, Moore, Propert, Stone, Turner, and Ware, and letters were received regretting their inability to attend, from Sir B. C. Brodie, Sir James M'Grigor, Drs. Babington, Bright, Chambers, Marshall Hall, and the Rev. Samuel Wix, and Messrs. B. Cabbell, J. T. Pratt, W. Tudor, A. B. Chisholm, and others.

Donations were announced to the amount £308 15s, being a much larger sum than usual. The present year, it was remarked by the Duke of Cambridge, was a bad one for charitable purposes; so the largeness of the collection was regarded as implying an increased appreciation of the merits of the institution.

NEWS OF THE WEEK.

RESIGNATION OF MR. GUTHRIE. — Mr. Guthrie tendered his resignation as surgeon to the Westminster Hospital at the board on Tuesday last. Mr. Hale Thomson at the same time resigned his appointment of assistant-surgeon with the full expectation of being placed in Mr. Guthrie's vacated place. It is pretty well understood that "Mr. Charles" will walk over the course, as, in fact, he carries the certainty of his success in his pocket. The affair altogether is rather funny. We stated some months ago that Mr. Guthrie, when vote-making, was proceeding at a rail-road pace at the hospital, came down to the institution and made a large batch of governors, under the idea that there was some plot going forward against his son. When he discovered the real state of the case he was in a "pretty considerable" rage at having been taken in. Since then we have heard that he has been so often laughed at respecting the matter, that he is determined the money shall not be lost, and hence his resignation.

CHANGES AT GUY'S.—Dr. Bright has resigned his office of senior physician to Guy's Hospital, and has been succeeded by Dr. Addison. Drs. Barlow and Rees are made physicians to the hospital, and Dr. Golding Bird assistant-physician. The state of Dr. Bright's health, which has been for some time declining, is the cause of his resignation.

APPOINTMENT AT ST. THOMAS'S. — Mr. M'Murdo has, *of course*, succeeded Mr. Tyrrell as surgeon to St. Thomas's Hospital.

THE paper-war respecting the dietary adopted in the Reading Gaol, has extended beyond the county of Berks, and is now being fought in *The Times*. One combatant in the newspaper asserts that the magistrates of Berks hope to continue their steady course of duty, equally with, or without, "newspaper opinions," and to adhere to the practice which experience has shown to be safe, &c. This *practice* appears to have consisted in the allowance of a certain amount of oatmeal-gruel in the food of the prisoners, and which Mr. Pearl characterised as liable to produce diarrhœa and debility; at any rate, in those who were unaccustomed to its use. But the grandiloquent combatant above quoted, who boasts of having shared the hospitality of a "noble duke" in the Highlands, seems to attribute the activity and the wiry sinew and muscle of the kilted inhabitants of those regions, to the oatmeal porridge which they used, instead of allowing that these existed in spite of their thin fare. He says, "Apathy, sullenness, or ferocity of temper, the seeds or advanced stages of disease, the conscience ill at ease, are all better reasons for *want of condition* (in the prisoners) than a few ounces of oatmeal." In short, the Reading justices have stubbornly refused to adopt even the *minimum* of food proposed by Sir James Graham, who has graciously declared that a deficiency of diet ought *not* to be made a means of punishment in gaols.—On this subject a correspondent of the *Times*, of Thursday last, demands,—"Has not enough of plebeian blood been shed in the course of these baleful experiments to warrant the framing of a law which shall arm the Executive against the obduracy of some of the magistrates!"

DR. FRANCIS WILLIS, in a second edition of his "Treatise on Mental Derangement," states that in most recent cases of the disease, even the most outrageous, the patient requires *support* and not *reduction* of nutrition, the malady being caused, as he co

siders, by a weakness, not an over-tension, of the nerves. He, however, advocates personal restraint, as both safe and merciful. He subdivides unsoundness of mind into three degrees: delirium, in which incoherent images and impressions pass uncontrollably over the mind; insanity, where the mind, though awake to present objects, is in a distorted position, from which it refuses to be moved; and derangement of mind—between the other two, though with difficulty distinguishable from the latter. He says little on the subject of monomania, and the responsibility attendant on the acts of those who are its supposed victims, but on this subject the following remarks have been made by a commentator:—Where we find that a disposition, originally suspicious, obstinate, reckless, and blood-thirsty, has, from indulgence of its propensities, darkened down into the twilight of insanity, and incited a man to outrage and crime, we ought to be cautious how we allow the belief that the urgency of his passions had arisen to disease so as to *exculpate* him from all guilt. A boy was tried for the murder of a companion before a French tribunal. Having been indulged by his mother, from childhood, in every bad passion, he had become, at last, totally unable to restrain himself from acts of violence, and was acquitted on the score of insanity—a dangerous and unsound decision. Considering such "morbid impulse" as only an exasperation of the natural disposition, should not a prisoner thus placed be held amenable to society for a crime committed remotely, though not immediately, in consequence of *responsible* depravity. For a criminal who has, by indulgence, reduced himself to commit crime, is, in a manner, during his admitted sanity, accessory to his own crime; and although, in giving way to his passions, it was not his intention that they should result in acts of atrocity, yet, acting on the provisions of the criminal law, we may view the homicidal monomaniac as a *separate individual, prompted by his former self*, and, even admitting the absence of intention, justly to be held liable to the criminal law. Blackstone lays down that he who in any wise commands or counsels another to do an unlawful act is accessory to all that ensues thereupon. Therefore the monomaniac, become criminal from indulgence, is an *accessory before the fact* to a crime afterwards perpetrated by himself—guilty in an equal degree with his principal, and punishable alike! This, however, is granting the supposition that he has lost the power of control over himself. But in an eloquent lecture at the Royal Institution on the 26th ult., the lecturer advanced that it was very rarely that in the insane the brain was found to be diseased in *every* part; and he adduced numerous instances to prove that in a majority of cases the patient was capable of exercising a control over his disposition, by abandoning the exercise of an organ, or a portion of the brain, that was phrenologically supposed to be unsound, for that of one which was, phrenologically, presumed to be sound.

A NATIVE of Bonn, named Meulen, a grocer's assistant, was lately cited before the police court at Strasbourg, charged not only with having exercised the healing art illegally, but with having charged ten times as much for his medicines as they could be bought for of an apothecary. The chief witness, who had been the patient, crawled forth to give his evidence, looking more like a living skeleton than the *terrâ imago divinis*. He stated that at the latter end of November, 1842, while suffering from a cough, he met, by accident, in a brewery, the accused, whom he had never before seen, and who told him that he had studied medicine in his native country, and promised soon to cure him. Next morning, Meulen sent his patient a dozen powders, ordering two to be taken daily, and charging 1s 4d for the packet. On the day following the patient said he was much worse. "That is just what I wanted," said Meulen, "you will be worse before you are better (empirics have a ready wit); you will soon be well." Far from this, however, the patient, who continued to take the powders, became affected with acute fever, marasmus, tic-doulouroux, and other evils, and was at last only saved from death by judicious medical treatment. At the police court, Mr. Grocer, confessing that he had no licence to practise, pleaded that the powders contained nothing injurious, but was fined £20. This species of summary justice affords a striking contrast to the process of law wielded by our apothecaries' companies. Another recent instance of the sharpness with which the French government look after adulterations affecting medicine has occurred in Paris, in the case of a manufacturer who had sold linseed-meal adulterated with bran to a druggist, the latter having retailed the same. The druggist was condemned to six days' imprisonment, a £20 fine, and the expenses. The manufacturer was sentenced to three months' imprisonment, and £2 fine, and the confiscation of the deteriorated drugs was ordered. Some appeals against this decision have, however, been set on foot.

BRUGES—decimated by misery and disease—has just expended 28,000 francs (£1100) on a statue to the inventor of the decimal calculus! Bruges contains 40,000 inhabitants, of whom 20,000 are paupers, and 10,000 more, without any fixed means of living, are almost without knowing how to obtain their next meal. Syphilis reigns triumphant in the city. At the civil hospital syphilitic patients are not admitted; no special dispensary for them exists, and the hospital surgeons and physicians, if they afford them advice, are summarily dismissed!

For individuals labouring under any other contagious disease the open air is their atmosphere. There is no children's hospital; the dispensary for eye-diseases is useless, and the maternity charity all but valueless; and for the aged and those with incurable complaints, the workhouse is the only refuge. Yet the communal council of the town has supplied men to set up a statue to the manes of Simon Stevens!

WESTERN GENERAL DISPENSARY.

To the Editor.—Sir: THE LANCET of May 21st contains a notice of the intention to build an hospital in Paddington, which the writer very properly approves. In mentioning the necessity for such a house of reception for accidents in that densely populated neighbourhood, he points out the absurdity of the Western General Dispensary being supposed to supply the desideratum, inasmuch as (to conclude the argument at once) that that building is but "a house with beds," properly so called.

Now, I will not count up the great number of sufferers from accidents which in a few years only I have seen received into and tended at the "house with beds;" nor will I point out the greater ease with which benevolent gentlemen might raise a small hospital into a large one, than—passing by the small hospital, supported wholly by the neighbourhood—they could draw on that neighbourhood for the building and support of a large hospital. If any one has wished to deprive the poor of the benefit of the hospital department of the Western General Dispensary, let him, in his repentence, empty his pockets for the building of a new hospital; but if there be any one who sees the necessity for further accommodation for accidents than the dispensary now affords, let him, in order to increase the amount of accommodation, aid those who have benevolently bestowed on the suffering poor whatever benefits they have hitherto received therefrom. Relying on your usual impartiality, allow me to subscribe myself a surgeon, but

NOT A SURGEON OF THE DISPENSARY.

June 1, 1843.

THE BULLY AGAIN.

To the Editor of THE LANCET.

SIR,—Will you do me the favour to publish the accompanying letter, received at the York Dispensary, on May the 10th last, from Dr. Dickson. It was only placed in my hands on Saturday, a serious accident having, up to that date, prevented me from attending to my duties at the dispensary. I am, Sir, your's respectfully,

T. LAYCOCK.

York, June 2, 1843.

"*To the Governor and Subscribers of the York Dispensary.*

"Gentlemen,—Herewith I forward to you a charge of piracy and literary swindling against one of your physicians, Dr. Laycock, which, for the honour of your charity, ought to be immediately investigated. To that person I have also forwarded a copy. I have the honour to be, Gentlemen, your most obedient servant,

"S. DICKSON.

"Clarges-street, April 6, 1843."

ROYAL COLLEGE OF SURGEONS IN LONDON.

LIST of gentlemen admitted members on Friday, June 2, 1843:—E. B. Clayton, J. A. W. Thompson, W. Gompertz, T. B. Stone, C. H. F. Routh, J. T. Pearce, J. Cross, J. W. Wainwright, R. Field, M. F. Bush, T. Berryman, F. H. Hewitt, J. C. Foulkes.—Admitted on Monday, June 5, 1843:—G. Lister, J. H. Blount, J. Dulgan, A. B. Semple, T. Dawson, W. Smith, W. W. Clarke, W. C. Northey.

CORRESPONDENTS.

The author of the letter signed *A Persecuted Pupil*, surely must be aware that we should not publish the statements in his letter unless they were authenticated, at least confidentially, by a real name and address. We should like, moreover, to see one of the original cards, bills, or notices, to which reference is made in the letter.

Non-Medicus.—The "effects on the constitution" can only be termed negative. Our correspondent might as well ask what are the effects of *not* taking a particular poison. We could better say what would probably be the consequences of *taking it*. The habit in question is neither necessary nor useful to the eaters of animal food in any quantity. Evidence on the subject is not merely of a conjectural kind. The facts are supplied by animal chemistry.

We can make no use of the meagre and unauthenticated note relating to the proceedings at the Ludlow Dispensary, having previously heard nothing respecting them.

W. Y. may practice at C. with the qualification that he names.

Studens does not say to what sort of "surgeons," he refers, whether possessing what constitutes a legal qualification at home or not. Inquiry on the subject should be made at the Colonial Office, in Downing-street.

B. L. is informed that there is no stamped edition, but THE LANCET can be obtained through the nearest bookseller.